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THE SUN.

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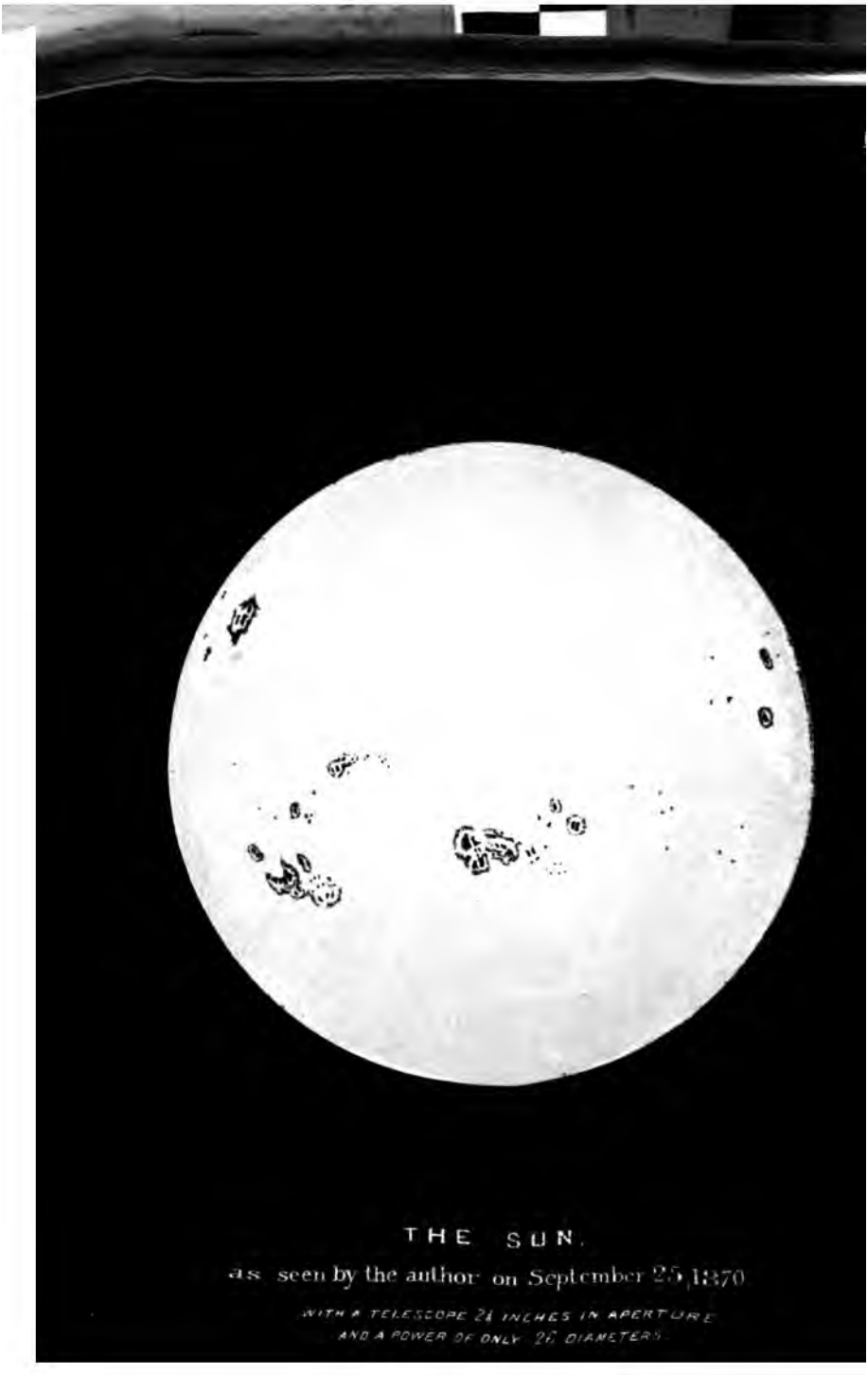


THE SUN.

as seen by the author on September 25, 1870

WITH A TELESCOPE 2½ INCHES IN APERTURE
AND A POWER OF ONLY 26 DIAMETERS.

THE SUN:

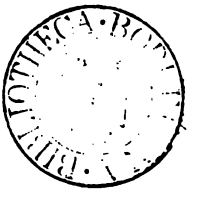
RULER, FIRE, LIGHT, AND LIFE OF THE PLANETARY SYSTEM.

BY

RICHARD A. PROCTOR,

AUTHOR OF 'OTHER WORLDS THAN OURS,' 'SATURN AND ITS SYSTEM,' 'LIGHT SCIENCE FOR LEISURE HOURS,' ETC.

Beyond expression bright
Compared with aught on earth, metal or stone;
Not all parts like, but all alike inform'd
With radiant light, as glowing iron with fire....
* * * *
What wonder then if fields and regions there
Breathe forth elixir pure, and rivers run
Potable gold, when with one virtuous touch
Th' arch-chemic Sun, so far from us remote,
Produces, with terrestrial humour mix'd,
Here in the dark so many precious things?



WITH NINE LITHOGRAPHIC PLATES (SEVEN COLOURED) AND ONE HUNDRED DRAWINGS ON WOOD.

THIRD EDITION.

LONDON:
LONGMANS, GREEN, AND CO.
1876.

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184. f. 27.

PREFACE

TO

THE THIRD EDITION.

THIS EDITION has been carefully revised, and some important sections (including an account of Professor Langley's researches) have been added. The appendix on Transits of Venus has been removed, the purpose for which that essay had been added having been fortunately accomplished.

RICHARD A. PROCTOR.

NEW YORK: 1876.

FROM THE PREFACE TO THE SECOND EDITION.

THE favourable reception accorded to the first edition of this treatise (two thousand copies of which were sold during less than a year from the date of publication) encourages me to hope that the work has been found a useful compendium of what is known or may be surmised respecting the great ruling luminary of our system.

The present edition has been completely revised, and in some important respects modified. Several new illustrations, for which I am indebted to Mr. Brothers of Manchester, and to Professor Young, Mr. Willard, and Mr. Knight, of America, have been added, as well as an account and analysis of the observations made during the eclipse of December 1870.

RICHARD A. PROCTOR.

BRIGHTON: *February* 1872.

PREFACE

TO

THE FIRST EDITION.

WHEN I had completed my treatise on Saturn and its System, I formed the design of preparing a separate treatise on each of the planets Mars and Jupiter, and then another and larger treatise on the Sun. Circumstances prevented me from carrying out this design at that time, and indeed threatened to withdraw my attention altogether from scientific pursuits. It is only quite recently that I have been able to resume my original design.

The delay has not been without its advantages, however. A work on the Sun has at the present time a far greater interest than it would have had four years since; while I have been able to obtain a much wider and more complete view of the subject than I should probably have thought necessary had I completed the work at that time.

My primary object in the present volume has been

to furnish a full account of the remarkable discoveries which have been effected by observers of the Sun, whether by means of the telescope, the spectroscope, polariscopic analysis, or photography. It will be seen that Chapters IV., V., and VI., in which I deal with these discoveries, together constitute more than one half of the main text. In these chapters the labours of the Herschels, Schwabe, Carrington, Secchi, De La Rue, Stewart, and others in examining the solar surface; the later observations of Huggins, Zöllner, Respighi, Secchi, Lockyer, Young, and others in the study of the prominences and chromatosphere; and the observations which have been made during the past two centuries on the phenomena presented by the solar corona, have been dealt with at considerable length.

But it seemed desirable that a separate and complete explanation should be given of all those matters which specially appertain to the application of spectroscopic analysis to the study of solar physics. Without an account of these matters, many of the most interesting discoveries made in recent times would be almost unintelligible, and it did not seem fitting to refer the general reader to the valuable but costly works of Roscoe and Schellen. Further, I think such a mode of treating spectroscopic analysis as I have adopted in Chapter III. of this work, more likely to be of use to the reader than a fuller but less simple account.

In one respect, indeed, Chapter III. presents what is wanting in every treatise on the analysis that I have hitherto seen: the matter in pp. 128–156 exhibits what really happens when the light from the object studied is sent through a battery of prisms. In Chapter III. I give an account of the principles of Browning's automatic spectroscope, and exhibit a plan of my own by which this principle may be extended so as to include a second battery. I think that in future applications of spectroscopic analysis, the plan illustrated at p. 139 is likely to be found of considerable utility.

Another large section of the book is devoted to the question of the Sun's distance. In Chapter I. will be found a full account (the fullest popular account yet published, I believe) of the researches which have been made up to the present time into this subject.

In Chapter II. a detailed account is given of the Sun's influence as ruler over the system of planets. Many of the relations here dealt with are novel and I think interesting. In the last three chapters I deal with the Sun's physical condition, his position as fire, life, and light of the solar system, and his place and motions among his fellow-suns. It will be seen that in Chapter VIII. I have presented reasons for considering that the most important work science has to accomplish is to show how the Sun's action can be more fully utilised than it is at present, so that before the Earth's stores of force are exhausted (as they must

some day be), resources which can never be exhausted —because unceasingly restored—may be rendered available.

It only remains for me to point out that in some of the notes I deal with matters and employ methods of treatment which would not be suitable for the main text. The general reader can omit these notes altogether, as they are not necessary for the elucidation of the subject, and have only been introduced for the benefit of those more advanced students of astronomy who might desire to see certain points more thoroughly dealt with than they could be in the body of a popular treatise like the present.

RICHARD A. PROCTOR.

LONDON: *December* 1870.

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THE SUN.

INTRODUCTION.

IT WOULD BE DIFFICULT to form an idea of the length of time during which the phenomena of day and night, and of the varying seasons, took place without attracting men's attention to the orb which governs both the day and the year. That the science now called Astronomy had its origin in the consideration of the Sun's apparent motions we can scarcely doubt. Clear indications remain, indeed, that the earliest efforts of men to determine the motions of the celestial bodies were directed to the great centre and ruler of the planetary scheme. But when those efforts were first made, what were the first conceptions of astronomers as to the nature of the solar motions, and how, in the process of time, those conceptions assumed the form described in the earliest records of astronomical research, we probably shall never know.

Nor, so far as my purpose in these pages is concerned, is it of any great importance that the truth in

these matters should be ascertained. It would doubtless form an interesting subject of study to trace the first progress of men as they endeavoured to elucidate the secrets of the heavens. Even what we know of the early researches of astronomers is full of interest, and not wanting in instruction. Their failures as well as their successes teach us a useful lesson of patience and of perseverance. The confidence with which at times they insisted on adopting erroneous theories may serve to teach us a lesson of modesty and caution. And the gradual process by which observation, and thoughtful reasoning upon observation, led men to the successful solution of so many noble problems, is as full of interest to the thoughtful student as the most spirit-stirring scenes of history. But where, as in the present instance, it is the object of the writer to exhibit the clearest possible picture of what is, it may be gravely questioned whether it is wise to present the full history of a series of researches which proceeded often on erroneous hypotheses. It is sufficiently difficult to convey in the compass of a single volume clear and accurate conceptions of a wide astronomical subject; and the task is rendered much more difficult where the history of false or imperfect theories is mixed up with the account of recognised truths.

But there is yet another reason for not undertaking to give in this work a history of the progress by which men attained their present conceptions respecting the Sun, nor even attempting the far easier task of showing how, by a series of simple observations, the position of

the Sun in the solar system, and especially with reference to our Earth, might have been ascertained. The fact is, that such an undertaking would differ but little from an attempt to combine a complete history of astronomy—or at the least a complete discussion of all known astronomical relations—with that special discussion of the Sun's nature and condition which forms the essential object of this treatise. For there is not a single chapter of a treatise on general astronomy which is not more or less associated with the relations presented by the Sun; and it may even be said that there is not a single subject dealt with by astronomers which does not owe its chief interest to such an association.

Therefore I find myself compelled to forego that mode of treating my subject which had seemed the best when I was dealing with the planet Saturn. One can, without prolixity, discuss the gradual progress of research by which the relations presented by a single planet have been, or might be, ascertained. But a preliminary research of this sort would require, in the case of the Sun, a volume—and no small one—to itself.

The course I propose, therefore, to adopt is as follows:—I shall pass over all that portion of the history of astronomy which relates to the determination of the Sun's central position in the system he governs. I shall give no account of the methods by which the nature of the Sun's diurnal motions was determined, nor shall I show how from this knowledge, combined with the gradual survey of the terrestrial globe and

the recognition of the Sun's apparent annual progress around the sidereal heavens, the ancients recognised the fact that either the Sun travels yearly around the Earth, and is carried also daily round with the heavens, or else that this Earth on which we live speeds yearly around the Sun, rotating daily on her axis. The observations on the planets by which the true interpretation of these apparent motions of the Sun was eventually obtained must also remain undiscussed.

We are to begin, then, by regarding the Sun as the recognised centre of the solar system, ruler over a scheme of worlds, on which he pours forth abundant supplies of heat and light.

So regarding the Sun, we shall first be led to inquire into the distance of the great luminary, in order that we may determine his real dimensions. The fundamental problem of astronomy—the determination of the Sun's distance—a problem which has at the present time a special interest on account of the transits of Venus in 1874 and 1882 from the observation of which astronomers hope to obtain new and better measures—will therefore form the subject of the opening chapter of this work. This chapter will naturally include the consideration of the Sun's dimensions, and of the scale of the solar system generally.

Then, next, the question of the Sun's mass, and of the influences which he exerts by reason of his mass, will come to be considered. We shall measure the might of the giant which rules the whole family of planets, and consider the limits of his domain. We

shall inquire what motions it is in his power to control at this or that distance, and so determine the limits of his power to gather fresh materials from out the surrounding spaces, either in such sort as to recruit his own mass, or as to enlarge the crowd of relatively minute bodies which circle continually, as we know, around him.

Then we shall proceed to discuss what the spectroscope has taught respecting the actual materials which constitute the Sun's substance, placing this inquiry before those chapters which deal with the telescopic aspect of the Sun's surface, the prominences, and the corona, in order that there may be no break in the narrative in passing from the era of unaided telescopic research to the recent era of mixed telescopic, spectroscopic, photographic, and polariscopic observation. In other words, an account of the principles of spectroscopic research, including so much of the history of spectroscopic analysis as is sufficient to make the subject clear (and therefore necessarily including an account of the Sun's general structure as indicated by the solar spectrum), will precede the narrative of that long series of researches which commenced with the discovery of the solar spots. The chapter on spectroscopic analysis will close with an explanation of those special modes of spectroscopic research which have seemed unintelligible, or rather incredible, to many, but yet depend on principles of exceeding simplicity.

Then will follow chapters describing the discoveries which have been made by aid of the telescope—and

its allies, the spectroscope, the polariscope, and photography—respecting the aspect and general condition of the Sun's photosphere, the coloured prominences, the corona, and so on. The physical condition of the Sun, the amount and probable source of his heat, and other like questions, will next be dealt with. And, finally, we shall consider the system of suns, and the position which our Sun holds in that system, so far as the researches yet made by astronomers enable us to deal with this noblest of all subjects of study.

Thus we have before us a sufficiently wide range of research. Our progress will lead us to consider some of the most successful attempts yet made by man to resolve the mysteries of the universe. We shall have to deal with much that invites reflection and speculation—with much that may be explained by the thoughtful study of evidence already obtained;—but also with much that continues, and may perchance continue for many years, altogether perplexing. We have, in fine, to deal with a subject which is full of interest, but whose real grandeur and significance, as well as its vast difficulties, are but now beginning to be rightly understood.

CHAPTER I.

THE SUN'S DISTANCE AND DIMENSIONS.

THE DETERMINATION of the Sun's distance is not only an important problem of general astronomy, but, so far as the subject of this treatise is concerned, it may be regarded as the very foundation of all our researches. For until we know the Sun's distance we can determine neither his bulk nor his weight; and our views even as to his physical condition will be found to depend in an important degree on the estimate we form respecting those two elements. A minute error in the solution of the problems on which the determination of the Sun's distance depends would not only result in adding or withdrawing hundreds of millions of cubic miles from the Sun's volume, and many multiples of the Earth's mass from his weight, but our conceptions of the size of solar spots, the height of the coloured prominences, the velocity of solar currents and cyclones, and many other such matters, would be rendered proportionately erroneous. It is, therefore, of the utmost importance that we should have accurate views respecting the modes of research by which the Sun's distance has been esti-

mated, and that we should know the probable limits of error in the resulting determinations. The subject has a special interest at the present time, because of the transits of the year 1874 and the year 1882. For a transit of Venus affords one of the most effective methods at the disposal of astronomers for the solution of this most noble problem.

Let us consider the general nature of the problem, in order that we may the better appreciate its difficulties. Many are disposed to wonder that astronomers should not long since have mastered what seems to be an almost elementary problem of the science; and it has appeared as a blot on the fair fame of astronomy that errors and differences of millions of miles should have followed the attempt to solve this particular problem. Then, too, when the resulting errors in the determination of the distances of the outer planets, and the still larger errors in the determination of the distances at which the fixed stars lie from us, are considered, the inquiry is suggested, 'Where is the boasted accuracy of the most exact of the sciences?' We shall see as we proceed that the great wonder is not that the estimates made by astronomers should differ, but that even the vaguest ideas should have been formed respecting the Sun's distance. The problem, as presented in its simplicity (the simplicity of perfect difficulty), seems at first sight one that no human ingenuity could avail to solve.

In fig. 1, let S represent the Sun, and E E′ E″ the orbit of the Earth. Then on the scale of the figure, a

dot meant to represent the Earth should have dimensions so minute that it would be altogether invisible to the naked eye. The line representing the Earth's path is broad enough to obliterate more than a hundred such dots placed side by side across its width. Now, it is the inhabitants of the globe represented

Fig. 1.

by this tiny dot, who have to measure the distance separating the inaccessible globe S from E E′ E″. The globe on which they live is continually rotating on its axis, it is sweeping onward with inconceivable velocity on the path E E′ E″; yet from this rotating, onward-rushing, and relatively minute orb, the observations are to be made by means of which the vast distance of S has to be determined. Still considering fig. 1, let us, in yet another way, picture to ourselves the nature of the problem to be dealt with.

Suppose the Earth on the orbit E E′ E″ to be represented by a globe one inch in diameter.* Then

* It is convenient to remember that a bronze halfpenny is exactly one inch in diameter; so that an exact representation of a great circle

that distance, and he will at once rec
cult a problem the astronomer has
termining the Sun's distance; for tl
scarcely perceptible disc subtends the
whose exact measurement the solution
depends. But even when anyone h
himself the difficulty of determining t
subtended by an inch disc at that dis
easily the angle might be over-estim
estimated by a considerable fraction of
—even then he will not have realised t
culty of the problem. Let him reverse t
and picture the difficulty of determin
vations made from points within this
distance of a station 320 yards off. Ye
will have underrated the difficulty of tl
blem astronomers have to solve. He
the two sets of observations to be mad
observers, at different seasons, in diff
with different instruments; that each
vations has to be corrected f

which must both be taken into account (involving a careful reference to the question of time) before any satisfactory use can be made of the observed results.

Such, in a general sense, is the nature of the problem astronomers have to deal with. The conditions of the problem are not of their fixing; all they can do is to face as resolutely and skilfully as they can the difficulties which the problem presents to them. They have done this so well that the history of the problem has become to the thoughtful student of science as interesting as a romance. But they do not pretend to have secured a greater amount of accuracy than the nature of the problem and the means at their disposal render possible. Let this be distinctly understood beforehand,—*Absolute accuracy in the solution of this problem is simply out of the question.*

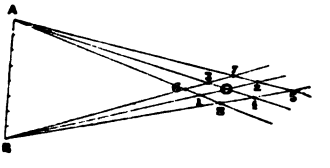
And now let us consider how the problem is to be attacked. In reality no less than six methods have been successfully applied; but there is only one method which is strictly geometrical in its nature, and this method must be the first to engage our attention.

In determining the distance of an inaccessible object, the geometer first measures a base-line, and then observes the bearing of the object from either end of that line. He thus has the means of determining the distance of the object with an exactness proportional to the accuracy of his observations.

Suppose, for example, the object is at O (fig. 2), and that the observer measures the base-line A B and observes the bearings B O and A O. Then, if he has done

this accurately, he can draw a picture, as in the figure, accurately representing his observations, and he can measure either A O or B O in this picture, and, by comparing these measurements with the length of A B in his picture, he can tell what relation the actual distances A O or B O bear to the actual base-line A B. Thus, if his base-line A B is 800 feet long, so that each of the divisions in the figure represents a length of 100 feet, then if B O be found to contain fifteen of these divisions, he will know that O is 1,500 feet from B. Usually, however, instead of merely drawing a figure

FIG. 2.



(a process in which errors may creep in through the imperfection of rulers, compasses, and so on), the surveyor would apply trigonometrical calculation to determine B O or A O as accurately as his observations permitted.

But now, if either his base-line or his bearings be wrongly determined, it is clear the distances A O, B O, will be wrongly estimated from them. The effect of a wrong measurement of the base-line is too obvious to need special discussion: clearly the error of B O or A O will be precisely proportional to the error of A B. But the error resulting from wrong estimates of the *bearings* requires to be attentively considered.

Suppose the bearing A O wrongly observed, and placed as A 1 or A 2. Then if the bearing B O be correctly observed, the resulting error will be O 1 or O 2 respectively. On the other hand, if the bearing A O be correctly observed but B O misplaced as B 3 or B 4, the error will be O 3 or O 4 respectively. If the bearings A O and B O be both misplaced outside, or both inside, the true direction of O, the place of the point O will be calculated as if at 5 or 6 respectively. And, finally, if the bearings are misplaced in different ways—that is, one inside O and the other outside—the point O will be calculated as if at 7 or 8 respectively.

Now, under favourable conditions, a skilful observer, though he must needs make *some* error in estimating his bearings (for no instruments can be absolutely perfect) would yet bring the estimated point relatively very near to O; in other words, though he might set it at a point out of place in the same way as any of the points 1, 2, 3, . . . 8, the *area of error* corresponding to the area 5 7 6 8 would be small compared with the area A O B.

But now suppose that instead of such a triangle as A O B, our observer has to deal with a triangle shaped like *a o b* in the next figure—an *ill-conditioned* triangle, to use an expression of Sir John Herschel's. It is at once seen that a very small error in either of his bearings will set the observer far wrong in his estimate of the distance of *o*. Suppose he has rightly determined the position of *a o*, but has the bearing *b o′* or *b o″* in place of the true bearing *b o*. He has the large error

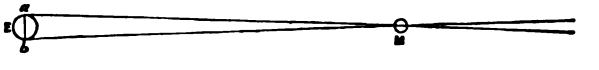
o o' or *o o''*, instead of the relatively small error 0 4 or 0 3 in the case pictured in fig. 2.

FIG. 3.

Now, the first important astronomical problem in distance-measuring—a problem infinitely less difficult than that of determining the Sun's distance—involves this very difficulty to a degree far greater than is indicated in fig. 3. I refer to the measurement of the Moon's distance.

If E (fig. 4) represent the Earth, the Moon would be placed somewhat as at M, and if it were possible to make use of two observatories situated as at *a* and *b* at opposite extremities of a diameter of the Earth, the actual difference of bearing of the Moon would be represented by the small angle *a* M *b*. As a matter of

FIG. 4.



fact, however, even this small angle has to be reduced considerably; because from *a* or *b*, the Moon would be on the horizon, and the estimate of her position rendered unsatisfactory by atmospheric refraction. The angle *a* M *b* is about a degree and a quarter, and it affords a very satisfactory idea of the skill with which ancient astronomers employed their relatively ineffective instrumental means, that their estimate of the Moon's

distance differed from the truth by only a fiftieth part. Modern astronomy has so completely mastered the problem of determining the Moon's distance, that the estimate now adopted can scarcely exceed or fall short of the truth by so much as twenty miles, or less than a ten-thousandth part of the whole.

But when the method thus shown to be available in the case of the Moon is applied to the Sun, it is found to be absolutely ineffective. The nicest observation fails to show any measurable difference in the Sun's position according as he is viewed from one or another part of the Earth's surface. It is true that there *is* a difference, and indeed a difference which is large compared with some quantities which astronomers are in the habit of dealing with; but as a means of estimating the Sun's distance, this direct reference to what is called parallactic displacement may be regarded as wholly ineffective.

Other methods, then, must be adopted. I proceed to consider two methods which suggested themselves to ancient astronomers. It is interesting to consider even those attempts which have failed; for they show the real difficulty of the problem we are engaged upon.

It occurred to Aristarchus of Samos (who flourished some twenty centuries ago) that the illumination of the Moon by the Sun affords a means of estimating the Sun's distance.

If *m* M′ M (fig. 5) represent the Moon's path about the Earth E, and S be the place of the Sun, we know

that the Moon is half full when near M. But clearly it is not when the Moon has reached the point M, such that M E *m* is a right angle, that she is exactly half full, but when she is at the point M′, such that E M′ S is a right angle. If, then, we can only determine the arc *m* M′, or find out how soon after new moon the Moon appears exactly half full, we can tell in what proportion the distance of the Sun exceeds the Moon's distance;

FIG. 5.

for in that case we have the angle M′ E S as well as the right angle at M′, and thus the shape of the triangle E M′ S is assigned, and with it the proportion of E S to E M′, which is what we require.

Let us pause to notice the ingenuity of this method. The point to be determined is, in reality, the distance between M and M′, or the angle M E M′, which is the same as the angle at S. In other words, instead of estimating the angle which the Earth's radius subtends as seen from S, this plan requires that we should determine the angle which the Moon's distance subtends as seen from S;—a much easier problem, first, because the latter angle is 60 times as great as the former, and secondly, because the necessary observations can be made at one terrestrial station.

Ingenious as the plan was, however, it was totally inadequate to meet the real (but as yet unknown) difficulty of the problem. Aristarchus estimated the

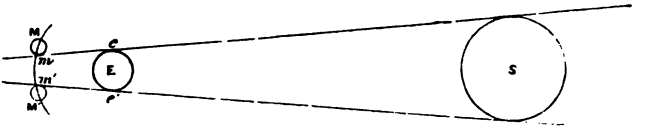
Sun's distance E S at nineteen times the Moon's, or (roughly) at a twentieth of its true value.

However, we should perhaps regard the estimate by Aristarchus as corresponding to those modern estimates of certain stellar distances, regarding which astronomers only say that they do not fall short of a certain value, without claiming to know how far they may exceed it.

The next plan of attack was devised by Hipparchus.

Let M (fig. 6) represent the Moon just entering the

Fig. 6.



shadow of the Earth E, S being the Sun. It is clear that if the Sun were just as large as the Earth, the shadow's width $m\ m'$ would be exactly equal to the Earth's diameter. If the Sun were less than the Earth, the shadow at $m\ m'$ would be wider than this; and if the Sun were greater than the Earth, the shadow at $m\ m'$ would be narrower than the Earth's diameter. Hipparchus reasoned that if $m\ m'$ were known, then by combining this measure with our knowledge of the Moon's distance and the Sun's apparent diameter, we could determine the Sun's distance.*

* We can determine at once the angle include l between e m and $e'\ m'$; it is easily seen that the angle subtended by the Sun's semi-diameter exceeds this angle by twice the Sun's horizontal parallax.

This method, like the former, was exceedingly ingenious, because it promised to enable a single observer, by merely timing the duration of a lunar eclipse, to solve a problem which, attacked directly, requires very delicate observations, made at stations very far apart.

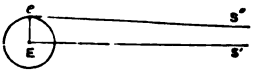
Again, however, the as yet unknown vastness of the Sun's distance foiled the ingenuity of astronomers. We now know that the plan just described is utterly inadequate; and we can readily understand how it was that Hipparchus and Ptolemy (who followed him in applying the method) arrived at a measure of the Sun's distance which lay no nearer to the truth than the determination made by Aristarchus of Samos.

Thus it came to pass that until the time of Tycho Brahe the received estimate of the Sun's distance was no greater than five millions of miles; nor is it too much to say that the methods applied by Aristarchus and Hipparchus might equally well have given any result whatever, from a million miles to infinity. In other words, the limits of error by these methods, and with the means available to ancient astronomers, actually exceed the quantity to be determined.

We come now, however, to the methods belonging to modern astronomy. Before dealing with them it will be convenient to indicate the quantity which—instead of the distance—is the *direct* object of the researches to be described. Of course the distance is what astronomers really require; but this distance is determined (at least as far as direct surveying methods

are concerned) by the measurement of the angle between lines directed towards the Sun's centre from different parts of the Earth. For convenience, one of the points is taken to be at the Earth's centre as E (fig. 7).

FIG. 7.



Now, if E S′ represent a line directed from E towards the Sun's centre, *e* S″ a line directed to the Sun from a point *e* on the Earth's surface, so placed that *e* S″ is an horizon-line (that is, square to E *e*), then the angle between the lines E S′ and *e* S″ is called the Sun's horizontal *parallax*,* and this is the quantity which astronomers set themselves to determine in the first place. Of course, the distance of the Sun becomes known so soon as this angle is determined; and throughout the remainder of this chapter, besides mentioning the parallax deduced by each method, I shall always mention the corresponding distance.

Six several methods have been devised, each at least as ingenious as the methods of Aristarchus and Hip-

* As the Earth is not a perfect sphere, horizontal parallax is different in different places. Further, the Earth's path is eccentric, and so there is a variation depending on her position in her orbit. To secure uniformity, the results obtained by astronomers are always referred to the horizontal parallax of the Sun at his mean distance and for a place on the Earth's equator—or the *mean equatorial horizontal solar parallax*, as it is called. It may perhaps be useful to remind the reader that this expression means merely the apparent length of half the greater diameter of the Earth's disc as seen from the Sun (at his mean distance').

parchus, and each requiring an exactness of observation which would have seemed to the old astronomers altogether hopeless of attainment.

The first two methods to be described are intimately associated with Kepler's laws of the planetary distances.

So long as no known law associated the distances of the planets from the Sun, it did not seem advisable to attempt to measure the distance of any planet from the Earth as a preliminary to determining the Sun's distance; for further observations were required in order to determine what relation the latter distance bore to the former. But so soon as Kepler proved that the distances and periods of the planets are associated by a simple law, it seemed a promising course to attack—instead of the Sun—some planet which approaches us within a less distance.

Let us consider, for example, the case of the planet Mars, in order that we may judge how much is to be gained by the course suggested. In doing this we are still following the actual order of events, for the first determination of the Sun's distance by modern astronomers, and with modern instrumental means, was founded on observations made upon the Planet of War.

In fig. 8, the orbits of Mars (M) and the Earth (E) about the Sun (S) are accurately laid down,—the line S ♈ representing a fixed line from which astronomers measure the motions of the planets around the Sun.*

* This line is only introduced to explain the unsymmetrical aspect of the two paths; to show, in fact, that they are intentionally eccentric.

Now, it is seen at once that when near M, Mars is much nearer to the Earth's path than the Sun is; so that when the Earth and Mars are in conjunction in this neighbourhood, it becomes easier (*pro tanto*) to determine the distance of Mars than that of the Sun. To put the matter simply, the Earth at E *looks larger* as seen from M than as seen from S; and to say she looks larger is the same as saying that she subtends a

FIG. 8.

greater visual angle; and the visual angle she subtends from Mars precisely measures the displacement which Mars will show as seen from different parts of the Earth.

Supposing Mars thus favourably situated, and that two observers, one at E and the other at E′ (fig. 9), observe the planet whose centre really lies at M, to lie on different points, *m* and *m′* of the celestial sphere. Then this arc *m m′*, if measured accurately,

would at once give the actual displacement of Mars corresponding to the distance E E′ between the observers; for though in the figure *m m′* is closer to M than to E, yet in reality the celestial sphere on which we thus estimate the place of Mars may be regarded as infinitely far off, so that M E is as a mere point at the centre of this sphere; and therefore the arc *m m′*, as estimated from the Earth, is precisely the same as though it were estimated from M,—or, in other words,

FIG. 9.

this arc measures the angle *m* M *m′* and therefore the equal angle E M E′. Thus we learn at once the angle E M E′, and as we know the base-line E E′, we deduce the distance E M by a very simple process of calculation.

But what is essential to the accuracy of the result is that the arc *m m′* should be accurately measured. Whatever error we make in this measurement will produce a proportionate error in our final estimate of the distance of Mars.

Now, if there were no stars in the background of the heavens, it would be absolutely impossible to measure *m m′* as accurately as our purpose requires. The problem would be quite as hopeless as the attempt to measure the Sun's distance by a similar process. But the presence of stars upon the celestial vault, and the certainty which the astronomer possesses that these

stars are at a distance incomparably exceeding that of Mars, make the measurement of this arc *m m′* feasible. The stars serve as index-points. In fig. 9, for example, a star is supposed to be placed at *s*; now while it would be hopeless for an astronomer to attempt to determine the direction of either line E M or E′ M′ in space, without reference to any star, it is quite easy to measure the arcs *m s* and *m′ s* with a very considerable degree of accuracy, and so to determine the difference *m m′*.*

And here one point in which the modern possesses an enormous advantage over the ancient astronomer, lies in the fact that spaces on the heavens which are blank so far as naked-eye vision is concerned, are shown by the powers of the telescope to be occupied by multitudes of minute stars,—and the minutest star serves quite as well as a large star for such observations as we are here considering. So that the astronomer need be under no anxiety lest Mars, during the period when he is nearest to us, should approach no star near enough to render the required measurements effective.

* Here I have supposed *s*, *m*, and *m′* to lie all on the same arc, which of course would not ordinarily be the case. It is easily seen, however, that it falls quite within the scope and bearing of ordinary astronomical observation to measure not only the distances but the bearings of *m* and *m′* from *s*, and so,—two sides and an included angle of the triangle *s m m′* being determined,—to determine the third side *m m′*. I may notice here in passing that quite a large proportion of the details involved in the various processes applied to the problem considered in this chapter are necessarily left untouched, or are barely mentioned. A volume much larger than the present would be required to exhibit these details in full and in all their bearings.

So soon as the distance of Mars has been calculated the distance of the Sun can be determined by the application of Kepler's third law. There is a preliminary process depending on the circumstance that S E and S M are not the mean distances of the Earth and Mars; but this process is perfectly simple, since observation has shown what is the true figure of each orbit. Thus Kepler's third law, by showing us the exact relation between the mean distances, shows us the exact relation between S M and S E; and therefore between E M and S E.

The plan here described was the first from which astronomers obtained any satisfactory estimate of the Sun's enormous distance. Kepler, after a careful study of Tycho Brahe's observations of Mars, had already confidently stated that the Sun's parallax is not greater than 1′ (or in other words that the Sun's distance is not less than 13½ million of miles). But Tycho Brahe's observations were such as we should now call altogether rough. Cassini proposed and carried out a much more exact series of observations. At his suggestion the Paris Academy of Sciences sent Richer to Cayenne, while Cassini himself, Römer, and Picard, observed Mars at different French stations. The parallax of Mars was indeed not measured, for the instrumental means of the observers were insufficient. But Cassini calculated that if the parallax had exceeded 25″, the means employed ought to have exhibited its effects. A parallax of 25″ in the case of Mars (situated as when Cassini's observations were

made) corresponds to a solar parallax of 10″. Cassini expressed his conviction that the solar parallax is not greater than 9″·5—in other words, that the Sun's distance is not less than 85,500,000 miles.*

The next application of this method involved the comparison of observations made by Lacaille at the Cape of Good Hope and by several astronomers at different European stations. The parallax deduced was 10″, corresponding to a distance of 81,700,000 miles.

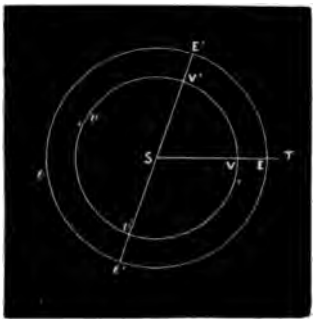
I shall presently have occasion to mention other and much more trustworthy results obtained by this method in recent times.† For the present, however, I pass on to other methods.

* Flamsteed, however, had already deduced a similar result by means of observations applied at a single station, according to the method described at p. 59. It would appear from his account that he had been the first to call the attention of the French astronomers to the favourable position of Mars. The observations of Flamsteed are remarkable as the first from which a definite value of the solar parallax was ever inferred. He assigned 10″, not as the superior limit of the parallax, but as its actual value. This corresponds to a distance of 81,700,000 miles.

† It might seem that as Mars comes into opposition at intervals averaging about 780 days, the method could be applied frequently, and so the results due to it could be rapidly improved upon. As a matter of fact, however, only those observations made when Mars is in opposition near perihelion are of service. From fig. 8 it will be seen that favourable opportunities do not occur at short intervals. The figure shows the successive conjunction-lines of the Earth and Mars between the years 1856 and 1871. It is seen that only the conjunctions of 1860 and 1862 are favourable, and *those* not so near as they should be to perihelion. (The wide distances separating conjunction-lines in this neighbourhood, as compared with the opposite, are due to the relatively rapid motion of Mars near perihelion.) The opposition of 1877 will be exceptionally favourable, as the conjunction-line will fall nearly midway between those of 1860 and 1862. It is necessary for me to remark that fig. 8 is copied from a drawing of my own, illustrating a paper in the *Popular Science Review* for January 1867.

The path of Venus lies even nearer to the Earth's orbit than that of Mars does. Fig. 10 represents the relation between the orbits E *e* and V *v* of the Earth and Venus; S ♈ as before representing the line from which astronomers measure the motions of the planets round the Sun. It will be seen by comparing fig. 8 with fig. 10 that when the Earth and Venus are in

FIG. 10.



conjunction as at E and V, the distance separating them bears a smaller proportion to the Earth's mean distance from the Sun than that separating the Earth and Mars when in conjunction. But then there is a circumstance in which Venus is less favourable for the purpose of astronomers than Mars. When the Earth and Mars are in conjunction as at E and M, fig. 8, the Sun is on the opposite side of the Earth, and so Mars is seen on a dark sky; but when Venus and the Earth are in conjunction, as in fig. 10, Venus lies directly towards the Sun, and even though visible (in powerful

telescopes), yet is seen under very unfavourable conditions. The background of the sky is bright, and none but the chief stars can be discerned, unless the telescope is very large and powerful, in which case it is not so well adapted as a more manageable one would be, for the class of observations required. Even the leading stars are but faintly seen; and as Venus may not lie near any of them, the kind of measurement which was available in the case of Mars becomes too precarious for the purpose of determining any parallactic displacement of the planet.

Hence the direct observation of Venus, when nearest to us, after the manner applied to Mars, is not a very valuable method of determining the Sun's distance. It may yet be applied successfully (according to the plan proposed in 1848 by Dr. Gerling, of Marburg); but even if it should, the method now to be considered is preferable.

When Venus and the Earth are in conjunction, she is not commonly on a direct line between the Earth and the Sun. She would be so, if the path V V′ *v* lay in the same level with the path E E′ *e*; but this is not the case. If we suppose the path E E′ *e* to lie in the plane of the paper, then the path V V′ *v*′ must be supposed to intersect that plane in the line *v*′ V′, the half of *v*′ V V′ lying slightly below the plane of the paper, and the half V′ *v v*′ slightly above,—the short lines near V and *v* showing the greatest amount of separation between the two planes on account of this tilt. Hence, unless the conjunction happen to take place when

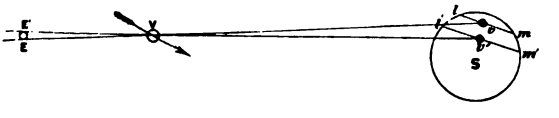
Venus is at v′ or *v*′, Venus will not be on a direct line joining the Earth and the Sun. But when Venus is within a certain distance of these two points,*—the nodes of her orbit, as they are called,—she will be so nearly on the line joining the Earth and the Sun,

* It is easy to calculate how near Venus must be to a node in order that she may be visible on the Sun's disc. The extreme limits will be those corresponding to the case when the disc of Venus as seen from all the Earth, save one point only, lies outside the Sun's disc, but as seen from that one point just touches the disc on the outside. Further, on account of the ellipticity of the two orbits, the exact extent of these limits will be different for conjunctions occurring when Venus is at v′ and at *v*′. To estimate these limits strictly, the exact values of the distances of Venus and the Earth from the Sun when in longitudes corresponding to s ε′ and s *e*′ may be taken from the *Nautical Almanac*, and combined with the estimated diameter of Venus. The formulæ to be used are sufficiently simple, and will occur at once to the mathematical reader,—or may be deduced from my paper on the transit of Venus in the *Monthly Notices* of the Astronomical Society for March 1869 (vol. xxix.). But for our present purpose such exactitude is not needed. It may be well, however, to determine in a general way the limits in question; for which purpose we may assume that no transit need be considered in which the line joining the centres of Venus and the Earth does not meet the Sun. Hence, Venus must not have a greater geocentric latitude than the Sun's apparent semi-diameter. Now the distance of Venus from the Earth, when she is in inferior conjunction, is to the Earth's distance from the Sun as 277 to 1,000. So that when her apparent latitude is equal to 16′ (which we may take as the Sun's semi-diameter) her heliocentric latitude is to 16′ as 277 to 723, or is about 6′·1. The inclination of her orbit being 3° 24′, or 204′, it is easily calculated that she must be within about $1\frac{3}{4}$° of a node, at the epoch of inferior conjunction, in order that a transit may occur. Thus at each node there is an arc of about $3\frac{1}{2}$° along any part of which Venus, if in inferior conjunction, will be projected on the Sun's disc; and as there are two nodes, the total range out of the 360° of her orbit along which transits can occur is about $6\frac{2}{3}$°, or a 54th part of the whole. Hence, on the average there will be one transit in 54 conjunctions, and as conjunctions occur at average intervals of 583·920 days, there will be on the average one transit in $86\frac{1}{2}$ years. But this average would only correspond to the case where a very large number of conjunctions was considered.

that as seen from the Earth she will appear on the Sun's disc.

Now when a conjunction of this sort takes place, the observation of Venus's apparent place is rendered much easier, since the disc of the Sun forms a sort of index-plate, as it were, on which we can estimate her position. So that if Venus's distance is to be determined at all from observations of her parallactic displacement as seen from different parts of the Earth's surface, it is

Fig. 11.



clear that the proper time for attacking the problem is when Venus is in transit.

Let us see, however, how this may be most effectually contrived.

Suppose that as seen from the point E (fig. 11) on the Earth at any moment, Venus (V) is seen at *v* on the Sun's disc; whereas as seen from E′ she appears to be at *v*′. Then it seems at first sight as though nothing could be simpler than to determine the distance *v v*′; and then having the arc *v v*′ and the length of the base-line E E′ we could at once determine the distance of Venus. For *v v*′ is the arc between two lines from Venus containing the angle *v* V *v*′, which is equal to E V E′. So that we know the angle E V E′. For example, if the arc *v v*′ were shown to be thirty-five seconds as seen from the Earth, then the angle *v* V *v*′

would be greater (because V is nearer to v v' than E is) in the proportion of E v to v V, or roughly as 7 to 5, so that the angle v V v' (or E V E′) is an angle of about forty-nine seconds. Now suppose that the stations E, E′ are so placed on the Earth's surface that the base-line E E′ is known to be about 6,000 miles in length: then the distance E V exceeds 6,000 miles in the proportion that the radius of an arc of forty-nine seconds exceeds that arc, or roughly as 4,200 to 1. Hence E V is about 25,200,000 miles, and E S exceeds this distance in the proportion of about 7 to 2, or comes out equal to nearly 90,000,000 miles.

But for many reasons this direct method of solving the problem of the Sun's distance has not been hitherto applied. In the first place, it is absolutely necessary that the observations made at E and E′ should either be made exactly at the same moment, or that the difference of time should be exactly known, so that the two observations may be fairly compared together. But for this purpose we must know the exact position of the stations E and E′ on the Earth, so as to be able from the apparent time at these stations to infer the true time at Greenwich or some other fixed station. It is easily seen that a very slight error in the determination of the longitude of either station would make the whole series of observations useless. For example, suppose the observer at E recorded the place of Venus on the Sun's disc at apparent noon for his station; and that the longitude of the station was *supposed* to be such that this epoch corresponded exactly with the epoch

when the observer at E′, recorded the Sun's position. Then if the supposition were correct, the above process would be available. But if E were ten or twelve miles to the east of the supposed longitude, apparent noon would occur a minute or so * earlier than at a place in that longitude. But, in one minute, Venus, as seen in transit, moves over an arc of about two seconds on the Sun's face, so that the observer at E noting her place a minute or so too soon (so far as the comparison with the other observer's record is concerned) would set her two seconds of arc out of place. But our problem is one in which seconds of arc are all-important.

But this is not all. The determination of the exact place of Venus on the Sun's disc at any epoch would be a matter of extreme difficulty. It would be necessary to determine, not merely her distance from the Sun's centre, but her bearing from that point; and a very slight error in either determination would (in so delicate an inquiry) cause a considerable error in the determination of the Sun's distance. Halley proposed (in 1716) a plan for evading the observational difficulties. He suggested, that instead of attempting to estimate the position of Venus on the Sun's disc at any moment, the observers at two stations, such as E and E′, should time the passage of Venus along her chord of transit. Neglecting for a moment the consideration of the Earth's rotation, Venus would seem to the observer at E to describe such a path as *l v m*, while to the ob-

* The exact difference of time would of course depend on the latitude of the station.

server at E′ she would seem to describe such a path as l' v' m'. Now if we know the length of time she takes in describing these chords, we know the length of the chords, since the rate of Venus's motion across the Sun's disc (the same of course for both stations on the assumption that the Earth is not rotating) is known from the tables independently of her actual distance. Hence it is a very simple problem in geometry to determine the distance separating the chords l m and l' m', and thence, as in the former case, to determine the distance of Venus, and so that of the Sun.

Nor does the fact that the Earth is rotating prevent us from applying this method; though it causes the problem to be somewhat more complicated. Venus in fact does not describe quite a straight chord across the Sun as seen from any station; nor does she move quite uniformly; nor again is her rate of motion across the Sun's face exactly the same as seen from different stations. But all these points are such as the astronomer is quite accustomed to take into account, nor do they in themselves detract one whit from the certainty with which Halley's method can be applied.

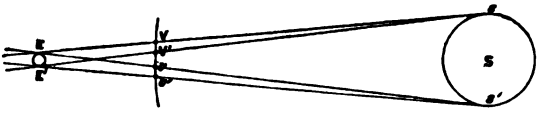
But there is one effect of the Earth's rotation which has to be very carefully considered in weighing the value of Halley's method. It is absolutely necessary (since the duration of the transit is to be timed) that at each station the beginning and end of the transit should be visible. Now a transit may last a considerable time—as long indeed as eight hours; and it may not always be easy to find two stations—one far to the

north, and the other far to the south, at each of which both the beginning and end of the transit will be favourably seen. For it must be remembered that a large part of the Earth is unfitted for the observer's purposes. We must not place our observers on the open sea, nor in regions where bad weather ordinarily prevails. And this question of the weather is in itself a great difficulty. For transits of Venus can only occur in December or in June, as is obvious from a consideration of fig. 10, where E′ and *e*′—the points near which the Earth must be that Venus in conjunction may be near a node—correspond to the Earth's position on about December 8 and June 6. Now, at a northern station in June, or at a southern station in December, fair weather may be commonly expected; but the reverse holds as respects the northern station in a December transit, and the southern station in a June transit. So that the difficulty of finding two stations, one northern the other southern, each well suited for observing both the beginning and end of the transits, and at each of which there is a fair prospect of clear weather at both epochs, is a very serious one.

The circumstances here considered are not such, ordinarily, as to prevent Halley's method from being applied, though they call for the most cautious exercise of judgment in the selection of stations for the purpose. The difficulty led Delisle, in anticipation of the transit of Venus in 1761, to propose another method.

Reverting to fig. 11, it will be clear that as V moves onward in the direction of the arrow,* the time must

FIG. 12.



come when the transit is just beginning at some point on the Earth's surface from whence the first view (as it were) is obtained of Venus in transit. Some interval must elapse before the transit has begun for the whole Earth—or at least for all that hemisphere which is turned towards the Sun. And at some point on the Earth's surface—which will clearly be nearly opposite the point just referred to—the transit will seem to begin later than at any other station. Now, neglecting matters of detail, and considering Venus as a point for the moment, we may reason in this way on observations of the kind considered:—

Suppose Venus at V (fig. 12) when the transit first begins, so that a line E V *s* just touches both the Earth and Sun, and that Venus is at V′ when the transit has begun for the whole Earth, so that E′ V′ *s* just touches both the Earth and Sun. Then we know the length of E E′, and therefore we know the length of V V′, which is less than E E′ in the proportion before used, of about five to seven. We also know exactly how long

* We suppose the Earth at rest, or—which is the same thing—consider only Venus's motion relatively to a line constantly joining the centres of the Sun and Earth.

Venus has taken to traverse this arc v v′, and therefore, since we know how long she takes to complete the circuit of her orbit, we know what proportion the known length v v′ bears to the circumference of her orbit. This gives the circumference, and thence the radius of her orbit, whence, as before, we learn the radius of the Earth's orbit.

Here we have supposed the Earth at rest. But as the motions both of the Earth and Venus are known, the relative motion of Venus is known, and so the conditions of the problem are as fully ascertained as in the simpler case actually dealt with.

We thus see that the phenomena presented at the commencement of the transit are sufficient for determining the Sun's distance. So also are the phenomena presented at the end of the transit; since it is obvious that as Venus passes from *v* to *v′* similar relations will be presented, but in a contrary order.

All that is required, then, for a successful application of Delisle's method is that one observer should have a favourable view of the commencement (*or* end) of the transit from some place on the Earth where the transit begins (or ends) nearly at the earliest, and that another should obtain a favourable view of the *same* phase at some place where the transit begins (or ends as the case may be) nearly at the latest. Both observers must time the commencement (or end) of the transit most carefully. Then to compare the two observations, in order to tell the absolute interval of time between the

two, we must know the exact longitude of the two places; for the observations will of course be referred to local time, so that in order to compare them, we must refer them to some standard time, as that of Greenwich or Paris.

Here, then, are the difficulties in Delisle's method: unless the longitude of each station is accurately known, and furthermore, the exact local time at which transit begins or ends, the determination of the Sun's distance will be inexact. As respects the former point there is little difficulty, only the observers must stay some time at their respective stations, making suitable observations to determine the longitude of the station. This can be done either before or after the transit, as may be convenient. But as respects the determination of the local time at which the transit begins (or ends as the case may be), there will be a difficulty if bad weather precede and follow the epoch at which the phase occurs. For we can only determine local time exactly when the weather is clear, so that we can make suitable observations on the stars; and during a few days of cloudy weather, the best astronomical time-pieces will get a second or two wrong. In Halley's method the clock may be altogether wrong, yet if its rate be fairly good, the duration of the transit will be accurately determined; but in Delisle's the clock must show absolutely correct time.

Here again, however, the difficulties, so far from being insuperable, are only such as astronomers are in the habit of dealing with and mastering.

Both methods were applied during the two transits of the eighteenth century. Of these one took place in June 1761, and the other in June 1769. Both occurring during the summer of the northern hemisphere, the Earth's northern pole was bowed towards the Sun; and in this respect the circumstances of the transits differed importantly from those of the transits in 1874 and 1882, both taking place in December, when the southern pole of the Earth is bowed towards the Sun.

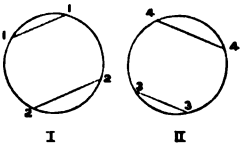
The transit of 1761 was not observed in a very satisfactory manner. It was a transit for the observation of which Delisle's method was somewhat better suited than Halley's,* and the astronomers of the eighteenth

* It is worthy of notice that in the case of two transits separated by an interval of eight years, the former is commonly best suited for Delisle's method, the latter for Halley's. Both the transits will occur at the same season, that is, Venus will either be near her ascending node at both transits, or near her descending node at both. For an interval of eight years corresponds almost exactly to thirteen revolutions of Venus, so that, supposing Venus near a node when in inferior conjunction, she will be near that node at the conjunction occurring eight years later (or after five synodical revolutions). Now, it so happens, that the line of these successive conjunctions in the same neighbourhood continually regredes round the ecliptic, one interval being in fact about $2\frac{1}{2}$ days less than eight years. Thus the latitude of Venus at successive eight yearly conjunctions near a node differs by the amount of Venus's motion in latitude (near a node) in the course of $2\frac{1}{2}$ days. Thus the apparent path of Venus across the Sun's disc (as supposed to be seen from the Earth's centre) would be as 1 1, 2 2, fig. 13 (I), when she is near her rising node or when both the transits are December ones; and as 3 3, 4 4, fig. 13 (II), when she is at a descending node, or when both the transits are June ones. [The distance between 1, 1 and 2, 2—and between 3, 3 and 4, 4, would be about that shown in the figure, and the slope with reference to the ecliptic is of course the same (approximately) in all transits. But the pair of lines may have any position whatever as

century were not so well prepared to deal with the difficulties of that method as those of our time will

respects distance from the Sun's centre. Now, if either line in either figure falls very near the centre of the Sun's disc, the other will not fall on the Sun, and there will be only one transit during those years when this conjunction is travelling past the node. This will happen in no inconsiderable proportion of these passages—a circumstance requiring notice because our treatises on astronomy commonly assert that transits of Venus occur at successive intervals of 8, 121½, 8, 105½, 8, 121½, &c. years, which is far from being strictly correct. In fact, instead of two transits occurring at every such passage, very nearly half the passages

FIG. 13.



would supply only one transit. The list in Lalande's Astronomy is wholly untrustworthy in this respect, as any one will find who will calculate the distance of Venus from her nodes at the conjunctions referred to in that list.] To resume:—Taking the case of December transits illustrated by fig. 13, we see that at the first transit when the path is as 1, 1, the northern station from which the path will appear lowest down on the Sun's disc will give the longest interval; and the advantage of applying Halley's method will depend on the greatness of this interval as compared with the shorter interval during which the transit lasts as seen from some southern station. Now the Earth is rotating, and the effect of the Earth's rotation *considered alone* is to give Venus a continual slight westerly displacement in all places where the Sun is moving from east to west—that is, at all places save those close by the south pole, at which the Sun (being above the horizon all day, or nearly all day) moves through part of the day from west to east. Hence at northern stations, where Venus's path is longest, she is hastened on her path by this westerly displacement; and so the lengthening of her period of transit is diminished and the value of Halley's method *pro tanto* reduced. At southern stations the shortening will be increased at places where the transit occurs during the mid-day hours, and diminished where the transit occurs during the midnight (nominal) hours.

prove themselves. Yet the result of the observations then made, which were interpreted as giving a solar parallax of 8″·65, corresponding to a mean distance of

But at the former stations the apparent path of Venus will not be thrown so far south as at the latter; so that at the southern stations also we find that the greatest possible shortening due to parallax cannot be combined with an additional shortening due to the Earth's motion of rotation. But at the second transit of this set, when Venus appears to follow such a path as 2, 2 (fig. 13), the reverse is the case. At the northern station Venus's path is thrown southwards and so shortened, while her motion across the Sun's disc is hastened (by the effects of the Earth's rotation) and therefore also shortened; whereas at the southern stations, where Venus's path is most lengthened, her motion across the Sun's face is retarded and so lengthened. Halley's method is then applicable under the most favourable conditions for securing a considerable time-difference. Similarly, it may be shown that at a June transit, when Venus's path is as 3, 3 (the first of a pair), Halley's method is not so favourably applicable as at a June transit when her path is as 4, 4 (the second of a pair).

Theoretically this is just, but practically, especially in December transits, the difficulty of securing suitable stations near the pole which is turned towards the Sun, may altogether change the conditions. As a matter of fact, indeed, the transits of 1874 and 1882 are exceptions to the rule; and I have been able to demonstrate that, so far from Halley's method being most favourably applicable in 1882 (as the Astronomer Royal had inferred from reasoning resembling the above), there is no reasonable chance of its being applied *at all* in 1882, the only two southern stations where it is possible to apply the method being such that the Sun will be barely 5° above the horizon,—a state of things preventing all exact observation, and assuredly not justifying expeditions to stations so near the south pole that the observing parties would inevitably have to winter there. On the other hand, I was also able to demonstrate that Halley's method, besides all the advantages arising from its simplicity, was applicable under more favourable circumstances than Delisle's, in 1874. There was nothing hypothetical about this conclusion. The difference between my conclusions and those before adopted arose simply from my having taken into consideration facts which had been (mistakenly) imagined to be such as might safely be neglected. After my results were published, the astronomers of the Continent and of America adopted arrangements in precise accordance with the views I had insisted upon.

about 94,500,000 miles,* was a great improvement on any before obtained—and better, in fact (though this was due to chance), than that deduced from the more complete and satisfactory observations made in 1769.

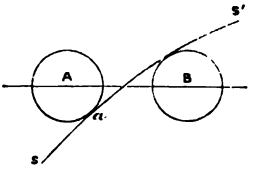
The transit of June 1769 attracted an amount of attention both in England and on the Continent which afforded very creditable evidence of the scientific enthusiasm of the men of the last century. The Royal Society presented a memorial to King George III., requesting that a vessel might be fitted out at Government expense to convey skilful observers to one of the stations which had been judged suitable for observing the phenomenon. The petition was complied with, and after some difficulty as to the choice of a leader, the ship Endeavour, of 370 tons burden, was placed under the command of Captain Cook. Continental astronomers also betook themselves to the most advantageous posts for observing the transit; and when at length Venus came between the Earth and the Sun, her arrival and passage were watched by observers at Wardhus, Kola, and Kajeneberg, at St. Petersburg, Orenberg, Yakutsk, Pekin, Manilla, Batavia, Otaheite, St. Joseph (in California), Kola, Hudson's Bay, and other well-chosen stations.

* It is convenient to notice that if the solar parallax were 10″, the distance of the Sun would be 81,738,420; and the distance corresponding to any other value of the parallax can be deduced by simply dividing 817,384,200 by the number expressing such parallax. The table in Ferguson's *Astronomy* is incorrect, owing to the erroneous estimate of the Earth's mean diameter on which the table is based.

Most of the observations of the transit were well and skilfully conducted; but a circumstance, which now for the first time attracted serious attention, caused a certain difficulty in their interpretation. The observers had to determine the exact interval of time occupied by Venus in crossing the Sun's disc—or else in certain cases the exact moment of time when she began to cross the solar disc. Now, if Venus presented the appearance of a mathematical point, the observation would be simply described in the words I have just used. But as Venus has a disc of appreciable dimensions, the question arises, whether the commencement or end of transit shall be considered with reference to the moment when the disc of Venus just touches the Sun's disc on the outside, or to the moment when her disc just touches his on the inside, or, finally, to the moment when her centre is crossing the Sun's edge. As to the last of these cases, we may dismiss it at once from consideration, because no observer, however experienced, could pretend to determine within a second or two when exactly one half of the disc of Venus was upon the Sun's. Either, then, the observer must note when Venus is, as at A (fig. 14), just touching the edge of the Sun's disc, S S′, on the outside, or, as at B, just touching that edge (the *limb* as astronomers call it) on the outside. Now, taking the case of ingress, if the observer knew exactly where Venus would begin to cross the Sun's disc, as at *a*, and very nearly the true instant of such ingress, he might, if the weather were very favourable, determine within a second or two the

moment when the uniformity of the limb s s′ was marred by the encroachment of the disc of Venus. But as a matter of fact neither one condition nor the other is fulfilled, and so, even under favourable circumstances of weather, he might not detect the exact commencement of ingress. Nor is this all. By favourable weather, I mean something more than clear weather. In weather which seems perfectly clear the telescopist will often see the edge of the Sun's disc absolutely *rippled* through the effects of atmospheric disturbance, and when the Sun is near the horizon,

Fig. 14.



even in good observing weather, the outline of the disc is often egregiously disturbed. Under such circumstances nothing would be more difficult than to assign the exact moment when external contact took place. But these are the very circumstances to be looked for in the observations necessary for determining the Sun's distance. The Sun *must* be low down at all the stations most suitable (in other respects). For the essential point both in Halley's method and Delisle's—nay, in any method whatever for determining the Sun's parallax—is that two observers shall be as far as possible from each other on the illuminated

hemisphere of the Earth; so that they must be near the *rim* of that hemisphere. In other words, they must be near that great circle of the Earth along which the Sun is seen exactly on the horizon. The Sun then will be nearly on the horizon at such stations, and will be viewed necessarily under somewhat unfavourable conditions.

It was this consideration which led, and probably will always lead, to the selection of internal contact as the proper phase to be observed. In attempting to time the moment when Venus is just within the Sun's disc, either at ingress or egress, there will, of course, be difficulties arising from atmospheric disturbances; but on the whole the observation of this phase is easier than the observation of external contact.

But, unfortunately, a certain peculiarity affects the appearance of Venus when just within the Sun's disc. Instead of appearing circular, she assumes (just before she leaves the Sun's edge at ingress, or just as she reaches it at egress) a pear-shaped aspect, as at 1 (fig. 15), or such an aspect, as at 2 or 3, according to

FIG. 15.

the nature of the telescope made use of, the conditions of the atmosphere, and the visual power of the observer. Thus the question arises, whether the assumption of this aspect is to be regarded as the true moment of

internal contact, or whether that is the true moment when the circular part of Venus's apparent outline, if continued so as to form a complete circle, would just touch the Sun's outline. Between one moment and the other several seconds occur, and the whole question is one of seconds.

Here is a difficulty of grave importance; for it is to be noticed that a practised eye would be needed to determine the moment when the outline of Venus would just touch the Sun's if undisturbed, and even a practised eye might well be deceived on such a point. On the other hand, though an observer might time the exact moment when the outline of Venus is just clear of the Sun's (and within it), either at ingress or egress, yet as this moment depends (as already mentioned) on the qualities of the telescope and on the observer's visual powers—probably also on the state of the weather —there is room for a considerable error to creep in.

I shall discuss further on the interpretation of the peculiarity just considered. At present the point to be attended to is the actual effect produced by this peculiarity upon the trustworthiness of the results obtained in 1769. The observers were for the most part unprepared for the serious difficulty which the peculiarity actually introduces. Although in 1761 Venus had been distorted at ingress and egress, while Halley himself had in 1753 noticed that Mercury exhibited a similar distortion, it had not occurred to astronomers to discuss the phenomenon, so as to be enabled to point out how the difficulty might be got

over. In fact, the whole matter had been so completely overlooked, that, as I have said, few of the observers in 1769 were prepared for the peculiarities which Venus presents when in interior contact with the Sun.*

The result was that much difficulty was experienced in interpreting the observations. It will be understood that more observations were made than were actually necessary for the solution of the problem. One pair of observations, if absolutely exact, would have sufficed to determine the Sun's distance; but as errors must to a greater or less extent affect all observations, it was necessary that many sets should be made in order that the mean result should be taken. Now, in all such cases the amount of reliance which is to be placed on the final or *mean* result depends on the closeness with which the several results aggregate round the mean. Precisely as we should place little reliance on the deduced mean of a series of ordinary measurements, which differed considerably *inter se*, so astronomers would not be prepared to accept with confidence a

* There is nothing more remarkable in the history of astronomical observations than the little preparation made for important occurrences, such as transits and eclipses, so far as the actual observation of the phenomena is concerned. Abundant preparation is made as far as instrumental means are concerned; but again and again the history of astronomy exhibits cases where the actual phenomena of transits and eclipses take the observer by surprise, and so are not observed as well as they might have been, even when abundant information has been in reality available for the instruction of those who are to take part in watching such occurrences. There is much, for example, in the history of eclipse-observations that is exceedingly painful to the real lover of science, more particularly in the wearisome repetition of observations which have already revealed all they can reveal, and in the apparent dearth of invention as respects the devisal of new modes of research.

value of the Sun's distance which was the mean of several discordant results.*

Now, the observations made in 1769 were in this respect much more satisfactory than those made in 1761; but, owing to the peculiarity I have mentioned, they did not accord so well by any means as might have been anticipated.

It is not too much to say that in the attempts first made to determine the Sun's distance by means of the transit observations of 1769, no settled principle was adopted. It was not much to be wondered at, therefore, that the estimates of the solar parallax varied between somewhat wide limits; so that whereas some mathematicians made the solar parallax as great as 9″·2, others obtained a value of only 7″·5. The corresponding values of the Sun's distance are respectively 87,890,780 and 108,984,560 miles,—a range compared with which recent discrepancies seem insignificant.†

* The total number of observers in 1761 was no less than 63—thus distributed: 13 in North Europe, 8 in England, 15 in France, 6 in Spain, Portugal, and Italy, 16 in Germany, and 3 in other places. In 1769 there were observers at 50 stations in Europe, 6 in Asia, 17 in America, and at one station in Polynesia.

† The results of the transit of 1761 are thus summed up in Dr. Bruhn's *Life of Encke* (*Johann Franz Encke, sein Leben und Wirken*):—Short obtained a parallax of between 8″·47 and 8″·52; Pingré, 10″; Rumowski, 8″·35; Planmann, 8″·2; Audefredy, 9″·2. From the transit of 1769, adds Bruhn, Wm. Smith deduced a parallax of 7″·5; Hornby, 8″·78; Pingré, 9″·2, 8″·88, and 8″·43; Lalande, 8″·8; Lexell, by Euler's method, between 8″·65 and 8″·86, whence he finally adopted 8″·8. The value of the Sun's distance corresponding to all these results can be at once deduced from Table II., at the end of this work; save only the distance corresponding to 7″·5 (in Bruhn's work it is written 7′·5) which is stated above.

During the years 1822 and 1824 Encke re-examined the whole series of observations made on the transits of 1761 and 1769. With wonderful patience, especially when we consider the nature of his materials, he combined together the results of no less than 149 observations made during the former transit, and as many made (at 75 stations) during the second. From the transit of 1761 he deduced for the Sun's parallax the value 8″·49, and from the transit of 1769 he deduced the value 8″·60; * while, by combining the two, he arrived at 8″·5776, the value which was employed during so many years in the 'Nautical Almanac' and other like works. The corresponding distance, viz., 95,274,000 miles, held its ground during all those years in popular treatises on astronomy.

It is somewhat surprising, considering the evidence which was afforded by the discrepancies between the observations made in 1761 and 1769, that this result should have been regarded with such confidence, since it needed but a brief examination of the basis on which Encke's result was founded to see that no faith whatever could be placed in three at least out of the five numerals in the expression 8″·5776. Delambre regarded 8″·6, very justly, as the most probable value of the solar parallax half a century ago. It was rightly

* The actual results deduced by Encke were—from the transit of 1761, 8″·490525 ± 0·060712″; from that of 1769, 8″·6030 ± 0″·0460; from the two combined, 8″·5776 ± 0″·0370—3·0120$\delta\rho$, where $\delta\rho$ is the correction for the Sun's semi-diameter (estimated by Encke at 958″·424). Later, Encke deduced the value 8″·5716, by introducing some corrections in his work.

admitted that the observations of Venus in transit afforded the most reliable results; but Delambre, Bessel, and other astronomers of eminence were far from adopting the value 8″·5776 with that implicit confidence which caused the corrections made in recent times to attract so much notice, and so greatly to surprise the general public.

The observation of Venus in transit being admittedly the most trustworthy method of determining the Sun's distance, it might have been supposed that no new results could serve to throw doubt on those deduced by Encke, until the time should come when Venus would again cross the Sun's face—that is, until the year 1874. But the rapid progress of science during the past half-century, though it has not served to alter the relative value of different methods for determining the Sun's distance, has enabled astronomers to apply some of the less powerful methods in so much more effective a manner than of old, as to obtain more trustworthy results than had followed from the best method of all when less skilfully applied.

Amongst these relatively inferior methods, no less than four are novel; but a very brief description must suffice for each.

It had been noticed by Laplace, towards the close of the last century, that among the perturbations of the Moon there is one which depends on the Sun's distance. Suppose M_1 M_2 M_3 M_4 to represent the Moon's path around the Earth, E. Then clearly as the Moon moves from M_4 through M_1 to M_2, she is disturbed by the Sun's

action, which is here greater on the Moon than on the more distant Earth. On the other hand, while the Moon is moving from M_2 through M_3 to M_4, she is disturbed, because the Sun's action is greater on the Earth than on her. Without entering into an exact investigation of the effects thus produced, it is abundantly evident that the Sun's perturbing effect in the former case will be greater than in the latter, because the radius of the orbit M_1 M_2 M_3 M_4 clearly bears a greater proportion to M_1 S than to M_3 S. The Moon's orbit is indeed so minute compared with the Sun's distance, that

FIG. 16.

the difference is very slight; but still there is a difference. When the Moon is at M_1 the Sun tends to pull her more strongly away from the Earth than he tends to pull the Earth away from the Moon when the latter is at M_3; and a similar preponderance holds for other and corresponding positions of the Moon and Earth. It follows that there is a slight variation in the Moon's motion depending on this cause alone, and readily admitting of being estimated theoretically, while the continued observations made by astronomers on the Moon's motions suffice to show how great the perturbation really is. It is only necessary to com-

pare the theoretical with the observed value to deduce the Sun's distance; only, of course, the accuracy of the result will depend on the number and accuracy of the observations. Laplace, with the best observations available in his time, deduced from the Sun's parallax the value 8″·6, corresponding closely with the value subsequently deduced by Encke from the transits of Venus. 'It is remarkable,' wrote Laplace, 'that an astronomer, without leaving his observatory, by merely comparing his observations with analysis, has thus been enabled to determine the distance of the Earth from the Sun—an element the knowledge of which has been the fruit of long and troublesome voyages in both hemispheres.'

But this method is clearly one which modern astronomers can apply much more effectively, because the observations at their disposal are so much more numerous, and so much more exact. Accordingly, Hansen, the eminent mathematician and lunarian, announced in 1854, in a letter addressed to the Astronomer Royal, that this method, applied with the aid of his new tables of the Moon, gave a solar parallax considerably exceeding Encke's estimate. In 1863, he assigned the value 8″9159, corresponding to a distance of 91,659,000 miles.

Another method, depending on the apparent motions of the Sun, was applied (with a very similar result) by Leverrier.

If the Earth had no satellite she would travel on her elliptic orbit round the Sun, with no other perturba-

tions than those produced by the planets. But since she has a satellite, whose mass is an appreciable though small aliquot part of her own, she is disturbed precisely in the same way, though not to the same extent, that the Moon is disturbed. The Moon travels once in a lunar month around her orbit, but the point round which the Moon moves is not the centre of the Earth, but the centre of gravity of the Earth and Moon; and around that centre of gravity the Earth also travels once in a lunar month. Now, precisely as an observer on the Moon would have in effect the range of the Moon's orbit around this centre of gravity, as a base-line by which to estimate the Sun's distance, so the observer on the Earth has the range of the Earth's orbit around the same centre of gravity for the same purpose. The diameter of this last-named orbit is indeed very small—little more, in fact, than three-fourths of the Earth's own diameter; but by the radius of this small orbit the Earth is sometimes in advance and sometimes behind her mean position. In other words, her motion in longitude (that is, her angular motion round the Sun) is not equable. Over and above the variation of her velocity due to the ellipticity of her path, there is this alternate advance and (relative) retrogression, having for its period a lunar month. Obviously, the observed effect, so far as the astronomer is concerned, is an apparent irregularity in the Sun's motion, having the same period of one lunation. The effect is exceedingly minute: it is less than the displacement of the Sun as seen from different parts of the Earth; and, as we

have seen, *this* effect could never be employed to determine the Sun's distance. Why then, it may be asked, is the other and smaller effect available? For this reason simply, that the daily observations made on the Sun in the meridian supply a fund of materials for estimating the effect in question. Such observations are made (severally) at one station by one telescope, and if not by one observer, yet by a series of observers who are always working together, so that their relative modes and powers of observation are comparable together.*

Leverrier, by the careful study of an enormous number of observations on the Sun, made at the principal observatories in Europe, came to the conclusion that the Sun's parallax is 8″·95, corresponding to a mean distance of 91,330,000 miles. Mr. Stone, however, has detected a numerical error in M. Leverrier's calculations; and when this error is corrected the value 8″·91 results, corresponding to a distance of 91,739,000 miles.† Mr. Simon Newcomb, of America, has, by the application of the same method, deduced the parallax 8″·81, corresponding to a distance of about 92,800,000 miles.

* The great point, however, is that all the observations are meridional. Were extra-meridional observations of the Sun as trustworthy as those made on the meridan, the Sun's distance could have been long since determined through those effects of the Earth's rotation which depend on the length of her diameter.

† Every method of solving the problem of the Sun's distance has its special difficulties. In Leverrier's method, the accuracy of the result is wholly dependent on the accuracy of our estimate of the Moon's mass; for clearly on this estimate depends the extent we are to assign to the

MM. Fizeau and Foucault applied a method differing wholly in character from any that had before been thought of. It seems at first sight incredible that the ingenious combination of revolving wheels or mirrors should serve to determine the Sun's distance; but such is the case. The essential point in the new method is the direct measurement of the velocity with which light travels. This velocity had been determined in two ways by astronomers, or rather it had been discovered in one way, and the deduced result had been confirmed in another. When Jupiter is in opposition —at which time he is nearest to us—the eclipses and occultations of his satellites were found to occur a few minutes earlier than had been calculated; whereas when he is near conjunction these phenomena occur after the calculated time. Römer first pointed out the meaning of this observation. He showed that the phenomena really occur at the calculated epochs, but

Earth's monthly orbital motion around the common centre of gravity of the Earth and Moon. But the Moon's mass is only measurable by observations determining the amount of the nutation of the Earth's axis, a quantity of the same minute order as the solar parallax itself. Still, this method has the advantage of depending on a very large number of observations, both as respects the determination of the Moon's nutation, and that of the inequality of the Sun's motion. It is obvious that the latter inequality may be employed either to determine the Moon's mass when the Sun's distance is known, or *vice versâ*. It has been employed both ways, Delambre having deduced the value of the Moon's mass by this mode. The way in which the inequality is applied will depend on the question whether the Moon's mass or the Sun's distance is supposed to be best known *by other methods*. At present it is assumed that the Moon's mass is the more accurately determined element; but doubtless after the transits of 1874 and 1882 the inequality in the Sun's motion in longitude will be applied to determine the Moon's mass from the known distance of the Sun.

that the light which brings to us the account of those phenomena reaches us more quickly when Jupiter is nearer to us than when he is farther away. Bradley afterwards found in this discovery the explanation of the aberration of the fixed stars. If light travelled with infinite velocity we should see the stars in the same direction whether the Earth was at rest or in motion. But as the velocity of light, though very great, is yet not infinite, the apparent direction in which the light from a star reaches the terrestrial observer is affected by the motion of the Earth, according to a law precisely similar to that which causes the apparent direction of the wind to be affected by the motion of an observer who is rapidly carried onwards in a carriage or other vehicle. And when Bradley determined the amount of the aberration of the fixed stars at different seasons, he found—and astronomers have since abundantly confirmed the result—that the precise velocity assigned to light by Römer was that required to account for the peculiarity which affects the apparent place of every star in the heavens, as the Earth sweeps onward on her yearly orbit.

But it will be seen that neither observation supplied the means of directly determining the velocity of light in miles per second. All that was known was—first, that light takes a certain interval of time in crossing the Earth's orbit (or some known chord of that orbit); and, secondly, that the velocity of light bears a certain proportion to the Earth's velocity in her orbit. Until the exact dimensions of the Earth's orbit are known,

neither of these facts informs us of the real velocity of light. Judging, however, from Encke's estimate of the Sun's distance, astronomers concluded that light travels at the rate of no less than 192,000 miles in a single second of time.

It might seem altogether hopeless to attempt to estimate directly a velocity so enormous as this. Remembering how the velocity of sound has been measured, and considering only the application of a similar method to the case of light, how utterly futile does the very thought of such an attempt appear! We can make a signal when a sound is heard at one station, and observers at another station can note how long the sound takes in reaching them; because where the stations are at a considerable distance, an appreciable time elapses before the sound travels from one to the other. But the very best signal we can use is some visible signal (in preference, I mean, to some electric signal), i.e., a light-message, which travels so quickly that we can wholly neglect the time it has taken, in comparison with that taken by the sound. Obviously, we have no such means of measuring the passage of light, for what is our signal to be if the velocity with which it is conveyed is so largely to exceed the velocity of light that we can neglect the time occupied in its transmission? But if we had even a satisfactory answer to this question (instead of having none whatever), the problem would yet be insoluble in this manner. Suppose the light were shown at a distance of 500 miles—about the limit at which any terrestrial light

could be seen, even under the most favourable atmospheric conditions—yet the time occupied by the light-waves in traversing this distance would be but about the 360th part of a second. What instrument or what observer could note such an interval—to say nothing of measuring it, which would yet be absolutely essential to the successful solution of the problem?

I shall not here enter into a full account of the means by which Foucault and Fizeau solved a problem apparently so intractable, referring the reader to 'Pouillet's Physics' and other works in which the subject of light is dealt with. The general principle of the method employed by Fizeau may be thus presented. Suppose we see an object by light-rays which have been caused to traverse a long path by means of several reflections. Now conceive that the continuity of the long path is simultaneously broken at regular intervals at two points, one near the beginning the other near the end of the path, the path being broken—re-made—broken—re-made, and so on. Then if light travelled with infinite velocity, the light-rays, which at any instant traversed the first part of the path at a time when the path was *made* there, would traverse the last part also, because at that same instant the path would be complete *there* also. But light not travelling with infinite velocity, the light-rays which pass the first part of the path may be stopped by the break in the second part, if the interval between the making and breaking be but short enough. Now, Fizeau had a revolving toothed wheel, and matters were so arranged that when a tooth

of this wheel was opposite a certain small aperture, the path of light was broken both at its beginning and end, for the light had to pass through this aperture, and then, after pursuing a long course, to pass out again through the same aperture. Thus when the revolution was moderately rapid, light-rays which passed through the aperture found the aperture open when they came back again to it; but by causing the revolution to be very rapid indeed, so that a very minute fraction of a second elapsed between the passage of successive teeth across the aperture, it was possible to cause the light-rays which had entered while the aperture was open to be prevented from passing out again by the interference of a tooth of the wheel. It is easily seen that when the wheel revolved at this particular rate there would be a total eclipse so far as light coming through the aperture was concerned. For light which went in when any portion of the aperture was free, would return to the aperture when just that portion of the aperture was closed. Then, as Fizeau had the means of telling at what rate the wheel was revolving when total eclipse thus occurred, he could tell precisely what fraction of a second elapsed between the passage of tooth after tooth across the aperture; and knowing the length of the path traversed by the light-rays, he could measure the velocity of light with a considerable degree of accuracy. Foucault adopted an arrangement in which a plane mirror was caused to rotate very rapidly, and the principle of his plan (which, to be fully explained, would require more space

than is here available) depends on the duration of visual impressions.* Fizeau's method, in some respects inferior to Foucault's, resulted in assigning to light a velocity of 194,600 miles per second; but Foucault's gave a velocity of only 185,300 miles per second, falling considerably short of the estimate of 192,000 miles above referred to. So satisfactory were Foucault's experiments, that this discrepancy was held gravely to affect the estimate of the Sun's distance, on which the latter value is based. It would follow, if Foucault's experiments were held to indicate truly the velocity of light in the interplanetary spaces (as well as in air), that the Sun's mean distance would be but 91,400,000 miles, his parallax 8″·942.

This method has recently been applied by Cornu, with results indicating a mean distance of about 92,000,000 miles.

The Astronomer Royal also suggested the applica-

* It may be thus exhibited:—An image of a certain wire is seen directly, and when a certain plane mirror which can be rapidly revolved is in a certain position, the image of the wire is caused to appear in coincidence with the wire seen directly. The mirror is so rotated as to take up once in each rotation the required position; and so long as the rotation is slow the reflected image makes successive appearances. With an increase in the velocity of rotation the image appears continuously in one place—owing to the continuance of visible impressions. Now, for moderate velocities of rotation, the position thus taken up by the image coincides with that of the wire seen directly. But with a very rapid rotation the position of the mirror suited for causing the image to be visible (after reflection), no longer accords appreciably with the position required when the mirror is at rest. Accordingly, the image appears appreciably separated from the wire seen directly; and the amount of separation, combined with the known velocity of rotation, supplies the means of estimating the velocity of light.

tion of a method already employed by Flamsteed, and later by Bond, of America. He pointed out that instead of comparing the position of Mars on the sky (when the planet is near opposition) as seen from different stations, an even more satisfactory estimate of the planet's distance, and so of the Sun's, might be obtained by observing how far the diurnal rotation of the Earth, by shifting the place of any fixed station, affected the apparent position of the planet. It is clear that if the station E (fig. 9) is supposed to be carried by the Earth's rotation to E′, the observer can as effectively compare the distances at which the observed places of Mars, *m* and *m′*, lie from a fixed star *s*, as though there were two observers, one at E and the other at E′ at the same instant of time; for astronomers know well how to take into due account the motion of Mars during the interval.

In 1862 this method was employed, as well as the former method of treating observations of Mars. The result was to confirm the impression that the Sun lies nearer to us than had been so long imagined. Stone, of Greenwich, by combining the two methods, deducing the solar parallax first from observations of Mars made at Greenwich alone, then from observations made at Greenwich and Capetown, then from observations made at Greenwich and Williamstown, and combining all these results, deduced a solar parallax of 8″·943, with a probable error of 0″·051. This corresponds to a distance of about 91,400,000 miles, with a probable error of about 500,000 miles. Winnecke, by combin-

ing observations of Mars made at Poulkowa and Capetown, deduced a parallax of 8″·964, corresponding to a distance of about 91,200,000 miles. Newcomb, from the observations of Mars in the same year, deduced a parallax of 8″·855, corresponding to a distance of about 92,300,000 miles.

Besides these, there were estimates by Leverrier, Stone, Pogson, and several others, founded on the re-examination of processes already referred to, or on observations enabling those processes to be applied anew with more or less chance of exactitude in the results.

By the year 1864 it had become abundantly clear that the accepted estimate of the Sun's distance was too great. All the new values clustered around a value of about 8″·9 from the parallax, corresponding to a distance of about 91,850,000 miles.

Thus astronomers were led to re-examine the observations of the transit of 1769 in order to see whether they could be so interpreted as to correspond with the result pointed to by so many independent researches. Powalky subjected these observations to a new discussion and deduced the value 8″·832, corresponding much more closely than Encke's with the recent determinations. But astronomers were not satisfied with his labours, because he rejected some of the more important observations made in 1769, without assigning sufficient cause, and also because the agreement between those observations which he retained was by no means exhibited satisfactorily. As Admiral Manners

said, when addressing the Astronomical Society in 1862, 'The weight of Encke's value could scarcely be said to be affected by Powalky's discussion.'

Mr. Stone, of Greenwich, next attempted to solve the difficulty which had so long perplexed astronomers. We have seen that there are two phases which an observer of Venus in transit might try to catch—the moment when the connection between the disc of Venus and that of the Sun appears to break at ingress or form at egress, and the moment when the outline of Venus, if undisturbed, would just touch the outline of the Sun. Now, if we consider why Venus is distorted so peculiarly near the time of internal contact, we shall see which of these phases corresponds most closely to the real moment of internal contact. The matter is sufficiently simple. Owing to a peculiarity of vision, every bright object appears somewhat larger than it really is, the borders of the image on the retina affecting the neighbouring part of the retina. It follows obviously that every dark object will appear somewhat smaller than it really is, for the background on which the dark object is depicted will be relatively bright, and so its image on the retina will enroach on that of the dark object. Thus, suppose the arc s s′ v′ (fig. 17) to represent part of the true outline of the Sun, then, to the observer, the outline would lie as shown outside this circle; but if the circle s′ v v′ represents the true outline of Venus, her apparent outline would lie as shown inside this circle. It is clear, then, that the part of the outline of Venus which lies upon the Sun seems

to belong to a smaller circle than the part of the outline lying outside the Sun. This is true until the actual moment when the true outlines touch, when the appearance would be that shown in fig. 18, the connecting ligament being the finest conceivable line; and obviously (referring to ingress), the undistorted

Fig. 17.

Fig. 18.

part of the apparent outline of Venus would have seemed to belong to a circle touching the Sun's outline some seconds earlier. Similar remarks apply to egress.

So that if an observer could note the moment when the discs of Venus and the Sun were apparently united by a fine line, as in fig. 18, he would have ascertained the moment of real contact; whereas if he had noticed the time when the circular part of Venus's outline would touch the Sun's if continued undistorted, he would have caught quite a distinct phase, which we may call apparent contact. As a matter of fact, only the finest possible telescope, and great care and observing skill on the part of the astronomer, would render it possible to notice the moment when such a ligament as is shown in fig. 18 broke or formed. Usually a much coarser ligament only would be noticed; but the

observation of the sudden breaking or formation of the ligament would be regarded as an approximate observation of real contact.

Now, Mr. Stone (after carefully inferring, from the account actually given by the different observers, whether real or apparent contact was noticed) introduced a definite correction,* according to the best estimate we have of the effects of irradiation in enlarging the solar disc and diminishing that of Venus.

By adopting this definite rule, Mr. Stone not only deduced a solar parallax corresponding closely with that obtained by the various other methods described above, but he brought the observations of the transit into most satisfactory accordance. Unfortunately the definite correction he introduced—about 17 seconds—did not in the least correspond with the *observed* time-interval between real and apparent contacts.

The result thus deduced assigned to the Sun a parallax of 8″·91, with a probable error of 0″·03, corresponding to a distance of about 91,730,000 miles, with a probable error of about 300,000 miles. In other words, the Sun's distance, according to this determination, is found to lie between about 92,030,000 miles and about 91,430,000 miles. If we assume 8″·9 as, on the whole, the best value for the parallax, implying, as it does, that as yet we have no knowledge of the next decimal figure, we have for the Sun's mean

* In this respect Mr. Stone's work differed from that of his predecessors in the same process of research.

distance 92,000,000 miles; as we ought not to admit any significant figures beyond the second. From the observation on the transit of 1874, the value of the parallax would seem to be about 8″·88, corresponding to a mean distance slightly exceeding 92,000,000 miles.

It remains to be noticed that the corresponding value of the Sun's diameter is fairly represented by 850,000 miles, nor is anything gained by expressing this diameter, as is so commonly done, with significant figures down to the units' place. Certainly if the true solar parallax is 8″·94000, for instance, then the exact value of the Sun's diameter is 852,908 miles. But when we see that the value of the solar parallax can only fairly be presented to the first decimal place, or as 8″·9, we cannot pretend to know the Sun's diameter with greater exactness. The limits of error in our estimate of the Sun's distance being probably four or five hundred thousand miles at the least, it must be admitted that the limits of error in our estimate of the Sun's diameter cannot be less than four thousand miles.

The surface of the Sun is equal to four times the area of a circle having this diameter of 850,000 miles, or in round numbers to 2,285,000 millions of miles. His volume is about 1,250,000 times that of the Earth. Of his mass and his resulting might as a ruler, I shall speak at length in the following pages.

In drawing to a close this chapter on the most difficult and important problem of astronomy—a chapter

which, long as it is, has barely sufficed to convey so much of the history of investigation as seemed essential to my purpose—I would remark that there is no branch of research with which astronomers have better reason to feel satisfied. We must not speak of recent successes as though they had removed a reproach from astronomy. There is no single part of the history of this problem of which astronomers have not abundant reason to be proud. By long and patient labours they have been able to overcome difficulties which might fairly have been thought insuperable. Availing themselves at every step of the best means they could secure for approximating more and more closely to the truth, they have necessarily had during their advance to pass from ground which they had formerly occupied. But every such change of ground has been an advance towards more complete success. Sir John Herschel has said that the recent correction in the value of the Sun's parallax, corresponds to the apparent breadth of a human hair at 125 feet, or of a sovereign at eight miles off; 'and that, moreover, *the error has been detected and the correction applied*; and that the detection and correction have originated with the *friends and not with the enemies of science.*' But I would go even farther than this, since in place of regarding the recent change as involving the detection and correction of error, I would speak of it rather as a new approximation, more successful indeed than former ones, but not therefore changing those former successes into defeats. The astronomy of half a century since had

as good reason to be proud of Encke's work as later astronomy has to rejoice at the successes of our own day; or as the astronomy of future ages will have to boast of those labours by which the results now accepted will inevitably be improved upon.

CHAPTER II.

THE SUN AS RULER.

ONE of the most important of the Sun's functions is that by virtue of which he rules the motions of his family of planets. By the exercise of his mighty attractive influence he continually controls the tendency which they have to rush tangentially far out into space beyond the influence of his illuminating, heating, and actinic rays. Their swift orbital motions, combined with the relative stability of their axial *pose*, result in producing the orderly succession of the seasons. But this succession would come to an end, were it not for the stability of their orbital motion; and this stability is due to the Sun's overmastering attraction. To this it is due that the paths of the planets though undergoing continual processes of variation, yet suffer no sudden changes as respects their distance from him, or, therefore, as respects the period necessary for a complete revolution. Nay, so perfect is the whole scheme of governance that even the processes of slow change take place within limits, and those limits not very wide. Not merely can the orbits of the planets suffer no sudden change, but they can neither suffer a great change nor a permanent change.

We might dwell much further on the importance of the Sun's influence as the most massive portion of the scheme of which he is the centre. On the one hand, we might point to the possibly even vital importance of the action which causes the terrestrial equinoxes to circuit the ecliptic in their grand processional year of 25,868 solar years, or of the slowly-exerted influence which changes the eccentricity of the Earth's orbit. It is very far from unlikely that but for these influences the Earth would long since have been rendered unfit, through a species of exhaustion, for being the abode of living creatures. But as it is, continents become oceans, and oceans continents, one hemisphere interchanges with another the office of supplying the chief proportion of land surface; activity is followed by rest and rest by activity; and so through countless cycles this globe has been and will continue to be a fit abode for innumerable races. On the other hand, we might dwell on the influence which the Sun's mighty attractive influences exert in gathering in from all sides abundant supplies of motive energy, to recruit it may well be, his seemingly exhaustless stores of heat and light and chemical activity.

But without dwelling further in this place on themes of which some will find a place in other chapters, while others—interesting and fascinating though they be—must yet be regarded as lying outside the range of our subject, let us proceed at once to consider what the Sun's true influence is, by virtue of that principle of gravitation which causes every particle of his mass to

aid in attracting all bodies within the sphere of his influence.

In one sense it may be said that the sphere of the Sun's influence is all space. According to the present conceptions of the power of gravitation, there is no particle of matter throughout the whole universe which does not feel the attractive influence of the Sun (as indeed of every particle of matter). But to all intents and purposes the Sun's reign may be regarded as limited. His influence on the stars is not merely minute so far as the amount of motion it is capable of producing in any given interval is concerned, but it is to be regarded as the influence of a peer among peers, not of a king over his subjects. The results of the mutual attractions of the stars may be, and doubtless are, of the utmost importance, but they do not belong to the history of the Sun as a ruler. On every side, then, the Sun's rule is limited—for in all directions there are stars, and the sphere over which each star rules, is as definite as that governed by the Sun, so that in each direction we come upon regions where his influence is subordinate to the influence of some other orb.*

* It is sometimes said that a body like a comet can pass from the sway of one star to come permanently under the dominion of our Sun or of another star, and *vice versâ*; but setting aside the case of interference with such a body through the action of a planet, or by reason of atmospheric resistance near the Sun, or the like, this can never happen. For let us suppose that a comet is passing from the sphere of one star's influence to that of the Sun's. Then it cannot be moving on a closed orbit around the Sun, at this time, for if so it is already subject to the solar dominion, contrary to our supposition. It must then be travelling on a hyperbolic or parabolic orbit around the Sun, and so must eventually

But it is desirable to know what sort of influence the Sun would exert even at distances beyond the limits of his direct control. Let us set ourselves to form adequate conceptions of the Sun's energy of gravitation.

The measure of all gravitation is that force which the Earth exerts as we know to draw bodies to her surface. We must therefore first ascertain what proportion the energy of the Sun's attraction bears to that familiar attraction exerted by the Earth.

For this purpose we may proceed either directly by comparing the amount of velocity which the Earth's attraction communicates to falling bodies, with the actual motion towards himself which the Sun causes in the case of any planet; or indirectly by comparing the motion of any planet round the Sun with the Moon's motion round the Earth. I select the second method as being the simpler.* I adopt also a way of

pass wholly away from his dominion. It follows that, setting aside the above-mentioned influences, a comet which travels in an unclosed orbit round one star can never travel in a closed orbit round any star, but will continue to flit from star to star through all time.

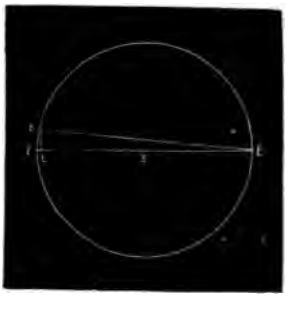
* It is worthy of notice, however, that the other method, though seldom applied, is in effect quite as available as the method in which reference is made to the Moon's motion. Thus we know that in one second a body falls about 16·1 feet towards the Earth, acquiring a velocity of twice this number of feet, or 32·2 feet per second. Now the Earth circuits the Sun at a rate of about eighteen miles per second, and supposing $e\,e'$ (fig. 19) to represent this distance on the orbit $e\,e'$ E, then e L, obtained by drawing a perpendicular from e' on the diameter e S E, represents the amount by which in effect the Earth has been drawn towards S, and a velocity of twice e L per second measures the Sun's gravity at the distance S e. But by Euclid vi. 8, $e\,e'$ is a mean proportional between e L and e E; in other words, e L is equal to $\frac{(e\,e')^2}{e\,\mathrm{E}}$; and therefore

applying it by which certain difficulties of conception are removed.

The Earth at a distance from the Moon of about 238,800 miles has power to change the direction of the Moon's motion through four right angles in 27·322 days, the Moon moving in her orbit round the Earth

the Sun's gravity at e is measured by a velocity of $\frac{(e\,e')^2}{e\,s}$ per second, or in feet $\frac{(18)^2 \times 1760 \times 3}{91,500,000}$. Hence at a distance equal to the Earth's radius, the Sun (supposing all his mass collected at one point) would exert a force represented by a velocity of about $\frac{(18)^2 \times 1760 \times 3 \times 91,500,000}{(4,000)^2}$ feet per second, which reduces to 9,783,180 feet (or about 1,853 miles per second). Comparing this with the measure of the force of terres-

FIG. 19.



trial gravity, or 32·2 feet, we see that the Sun's mass must be, according to this rough process, fully 300,000 times as great as the Earth's. As a matter of fact it is estimated at 315,000 times the Earth's, a result we should have closely approximated to had we taken the Earth's radius at 3,960 instead of 4,000.

The following general theorem is often useful. Take r the Earth's radius, E her mass and g terrestrial gravity, R the radius of a circular orbit described by a body of mass m, about a larger body of mass M with velocity v (in feet per second). Then the attraction between the bodies, or the gravity of m towards M, is represented by the expression $\frac{v^2}{R}$ and the mass $M + m$ is equal to $\frac{v^2 R}{r^2 g}$. If m is relatively very small

with a velocity which we may represent by $\frac{\text{[illegible]}}{27\ 3\text{[illegible]}}$.* Now the Sun at a distance from the Earth of about 91,500,000 miles has power to change the direction of her motion through four right angles in 365·256 days, the Earth moving in her orbit with a velocity which we may represent by $\frac{91,500,000}{365\ 256}$. Now clearly, since gravity varies inversely as the square of the distance, the Sun would require (were other things equal) to have an attractive power greater than the Earth's in the ratio $\left(\frac{91,500,000}{\text{[illegible]},000}\right)^2$ to produce the same effect on her that she produces on the Moon; and secondly, since to change the direction of a body's motion through any angle is a work which will be done at a rate proportioned to the force which operates, it is clear that the Sun's attractive power would have (were other things equal) to be less than the Earth's, in the ratio $\frac{27\ 3\text{[illegible]}}{365\ 256}$, to accomplish in one sidereal year what the Earth accomplishes in one synodical month; while lastly, since the faster a body moves the greater the force necessary to deflect its

(as in the case of the Earth compared with the Sun), and the radius of the larger mass is ρ, then gravity at the surface of the larger mass is equal to $\frac{v^2.R}{\rho^2}$.

It may be added that if v and v be the velocities with which bodies coming from an infinite distance reach the surface of orbs having radii R, r, and mean densities Δ, δ, then $\frac{v}{v} = \frac{R}{r}\sqrt{\frac{\Delta}{\delta}}$.

The same relation holds if v and v are the velocities with which bodies circle round these orbs just at their surface.

* We need not trouble ourselves to determine the velocity in miles per second, or minute, or hour; because relative and not absolute velocities are in question. Hence we can represent the Moon's velocity by the radius of her orbit divided by the period, provided we represent the Earth's velocity in like manner.

course through a given angle, it is obvious that the Sun's attractive power would have (were other things equal) to be greater than the Earth's in the proportion of $\frac{91,500,000}{365 \cdot 256}$ to $\frac{238,800}{27 \cdot 322}$—that is, in the ratio $\frac{91,500,000 \times 27 \cdot 322}{238,800 \times 365 \cdot 256}$—to produce a given change on the quickly moving Earth in the same time that the Earth produces such a change on the less swiftly moving Moon. Now we have only to combine these three proportions, which take into account every circumstance in which the Sun's action on the Earth differs from the Earth's action on the Moon, in order to deduce the relation between the Sun's real attractive energy, and the Earth's (at equal distances from the centre of either). This gives the proportion $\frac{(91,500,000)^3}{(238,800)^3} \times \frac{(27 \cdot 322)^2}{(365 \cdot 256)^2}$, which reduces to 314,798, in which proportion the Sun's mass exceeds the Earth's. We may take 315,000 (the value given in tables of the elements) as representing in round numbers the true proportion, which as it depends on the Sun's distance, cannot be determined so accurately that the last three figures of the number can be regarded as significant.

At equal distances, then, the Sun exerts 315,000 times as much force on any body as the Earth. So that if the Earth's mass were as great as the Sun's, her dimensions remaining unchanged, a mass which now weighs one pound would weigh more than 14½ tons. A man now of average weight would be crushed down by a weight of more than 20,000 tons. A body, if raised but a single inch and let fall, would strike the ground with a velocity three times as great as that of the swiftest express train.

But now that we have thus ascertained the proportion which the Sun's attractive energy bears to that exerted by the Earth, and so are able to measure the Sun's might as ruler over his system by direct comparison with the familiar force of terrestrial gravity, we must endeavour to form an estimate of the extent of force exerted by the Sun at different distances. We are to inquire what are the limits of the Sun's effective reign, not as regards distance alone, but as regards also the activity of matter,—that is, the velocity with which it is travelling.

We may begin with the Earth. We know that the Earth is completely subject to the Sun's attraction. Notwithstanding the inconceivable velocity with which she moves, and therefore the inconceivable energy of the tendency she has to travel onwards in a right line, and so to free herself from the Sun's control, she is compelled to travel in a nearly circular course around him. At one time her velocity has reached its maximum and she has power for awhile to increase her distance from the Sun. But she has derived that very power from him. Anon her speed is reduced to its minimum, and then she is compelled slowly to approach the ruling centre. But throughout her course there is one constant relation from which there is no escape. The Earth's velocity and distance are the two quantities which measure the extent of the Earth's partial freedom. When one is reduced, the other is increased, and *vice versâ*, in such sort that there is absolutely no change in her condition regarded as

depending on these two combined relations. The one law from which there is no escape, let distance and velocity change as they may during the Earth's circuit around the Sun, is that her period of revolution continues unchangeable,—and therefore her mean distance also.*

We may then take the Earth's mean distance as measuring her freedom from complete solar control—complete control being understood to be such an overwhelming influence on the Earth as would force her to fall directly upon the Sun. And while the Earth's mean distance thus measures her partial freedom, the shortness of the period in which she completes her circuit measures the amount of the Sun's power over her. Her mass may be regarded as having nothing to do with either relation. Increase of mass, so far as it would be effective at all, would tend to increase the strength of the bond uniting the Earth and the Sun, and to diminish the period of the Earth's circuit. But remembering that the Earth's mass is a very minute fraction of the Sun's, it may be disregarded wholly. A body no larger and heavier than a peppercorn, if projected with the same velocity and on the same course as the Earth, would continue to travel in precisely the same path and period around the Sun.

* Or we may say in preference, that the one law from which there is no escape, is the law connecting the Earth's velocity v at any time, with her distance r from the Sun at that time, and a certain fixed quantity a which is her mean distance. This law is thus expressed mathematically: $v^2 = \mu\left(\frac{2}{r} - \frac{1}{a}\right)$, where μ is the accelerating force of the Sun at the unit of distance.

We have next a law for our guidance which is of a very remarkable character, and the recognition of which will be found to throw a most important light on all the relations of the planetary scheme. It is this: Given the distance of a body from the Sun, and the velocity with which the body is travelling, then—let the course of the body be what it may (so long only as it does not bring the body into actual contact with the Sun), the period of the body's revolution is assigned.

The Earth's greatest velocity and her least correspond therefore—as truly as her mean velocity—with her period; and further, we need not trouble ourselves about the direction of her motion at any time, for if this direction were altered by the action of some external force, while yet her velocity remained unchanged, she would continue to travel in the same periodic time around the Sun, and at the same mean distance.

So that if we take the Earth's greatest velocity when she is in perihelion (18·5 miles per second), we have the velocity which is necessary in order that a body about 90,000,000 miles from the Sun may travel once in a year, or at a mean distance of 91,500,000 miles round the Sun; and if we take her least velocity (17·9 miles per second) when she is in aphelion, we have the velocity which is necessary in order that a body about 93,000,000 miles from the Sun may travel once in a year round the Sun; while the Earth's mean velocity (18·2 miles per second) at her mean distance is the velocity necessary in order that a body at that distance may have a period of one year.

We will now take only the mean distance and the mean velocity. We see that at a distance of 91,500,000 miles a body requires a velocity of 18·2 miles per second if it is to have a mean distance of the same amount. So that clearly if it were projected square to a line from the Sun it would never change its distance; for it would have exactly the right velocity and exactly the right direction for travelling in a circle around the Sun. The Earth when at this distance, though travelling with the right velocity for the required mean distance, is not travelling square to a line from the Sun, and so does not travel in a circle around him. But we have learned from her motion what is the just rate at which a body should be projected so as to travel in a circle round the Sun at a distance of 91,500,000 miles.

Now how much must this velocity be increased in order to enable a body at this distance of 91,500,000 miles to pass wholly from the control of the Sun? If we can determine this we shall have determined the limits of the Sun's influence at this particular distance. Over bodies moving with a velocity below that limiting velocity he is completely master; let them travel onwards as they may, increasing their distance from him more and more, there is yet a limit to this increase. Their absolute velocity will become less and less; it will become at last such, that if their direction of motion were but changed they would thenceforth continue to describe a circle around the Sun; still it will go on diminishing, until at length they reach their

extreme range of distance, after which they will be brought back through all the orders of distance they have passed through, and finally return to the place they started from, to pursue the same round for ever. But if their velocity do but equal or exceed the limit we are dealing with, they will travel onwards—with ever-diminishing velocity, it is true, but still—with ever-increasing distance, *for ever.*

It might be supposed that a very great increase of velocity would be required in order that the Earth should be thus (unfortunately for her inhabitants) released from the Sun's service and sent to wander freely through space. But in reality it would not even be necessary that her velocity should be doubled; an increase by one-half would be more than sufficient to free the Earth for ever from enforced periodic revolution around the centre of our planetary scheme. The exact proportion of increase necessary to effect this is represented by the proportion in which the diagonal of a square exceeds the side,* which we know to be repre-

* The following simple formula conveniently expresses the relation between the mean distances a and a' of bodies which at a distance r from the Sun are travelling with the velocities v and v' respectively:

$$a'(2a-r) : a(2a'-r) :: v^2 : v'^2.$$

Now supposing the velocity v to be such that a circle would be described about the centre of motion, if the body travelled square to the line from that centre, then obviously a is equal to r; so that the above relation becomes

$$a' : 2a'-r :: v^2 : v'^2;$$

and therefore

$$a' : r :: v^2 : 2v^2-v'^2.$$

Now clearly if we increase v' until v'^2 is equal to $2v^2$, we make the fourth term gradually diminish until it vanishes; so that the third at last

sented numerically by the proportion in which 1414 (pretty nearly) exceeds 1000.

bears an infinitely great proportion to the fourth. But in this case the first will bear an infinitely great proportion to the second: in other words, a' will be infinitely great. Hence the period of the body will become also infinite (by Kepler's third law): or in other words, the body will never return to its starting-place. Therefore, it follows that if v be the velocity with which a body will describe a circle at a distance r from the Sun, the greatest possible velocity which a body can have at a distance r from the Sun, so as to travel on a closed orbit around him, is $v\sqrt{2}$; and if a body is observed to travel with any greater velocity at such a distance we know with certainty that that body has entered the domain of the Sun, with a velocity imparted to it by extra-solar influences.

If in the above proportion v'^2 is greater than $2v^2$, we see that a' must have a negative value. This means that the centre of the path described by the body lies in the direction contrary to that in which the Sun lies. And knowing that the path must needs be a conic section with the Sun in a focus, it follows that the path of the body must be hyperbolic, the Sun lying at that focus which is next to the branch traversed by the body.

Of course in this case as in the former the body will never return. But there is a noteworthy distinction between the two cases. When the axis of the orbit is infinite, the body describes a parabola, and if it could be traced from the Sun as it approached from an indefinitely great distance to its nearest point and then passed away again to an infinitely great distance, the point to which it seemed to pass away would be precisely the same as that from which it seemed to come, and these coincident points would lie directly opposite the point of nearest approach. (This is obvious, because if lines be drawn from the focus of the parabola to two points of the curve equally and enormously removed from the vertex, these lines will enclose an indefinitely small angle, and will approach indefinitely near to coincidence with the axis.) On the other hand, a body approaching and then passing away on a hyperbolic orbit will seem to come from one point of the heavens and to pass away to a different point, the bisection of the celestial arc between these points lying directly opposite the point at which the body makes its nearest approach; and for a given distance of this nearest point, the arc separating the two former will be the greater as the velocity of the body when at its nearest is greater; becoming equal to two right angles when this velocity is infinite'y great.

So that if the Earth when at her mean distance had her velocity suddenly increased from 18·2 miles per second, to 25·7 miles or thereabouts, we should be carried thenceforth continually farther and farther away from the light and life of the planetary scheme. From Mars and Jupiter, and perchance from Saturn, Uranus, and Neptune, the unhappy career of the Earth might be traced for many a long year (though years—at least terrestrial ones—would then be no more). But long before the Earth crossed the confines of those distant regions along which the outer planets pursue their career, all the higher forms of life would have vanished from her surface. She would still rotate; day and night would still succeed each other on her surface; but the orderly sequence of the seasons would be replaced by the continual diminution of solar light and heat, until a cold more intense than that of the bitterest Arctic winter would bind the world in everlasting frost.

A similar fate would befall us if the Sun's mass were suddenly reduced by one-half; the only difference being that in this case we should have companions in our troubles; for Mars and Venus and Mercury would all forthwith start on parabolic paths, carrying them away to infinite distances from the Sun. Nor would the larger planets escape. From their distant orbits they would rush off into outer space, carrying their systems of satellites with them; so that if I have been right in regarding these orbs as acting the part of secondary suns to their satellites, the latter would be less unfortunate in their fate than the four minor

planets, for these would have no sun at all, while the former would still enjoy such heat and light as their ruling centres could supply to them.

It is not without a purpose that I have thus dwelt on the general result of a sudden diminution of the Sun's mass. The consideration that all the planets would thus at once be freed from their allegiance if the Sun's mass were reduced, leads us to the consideration that each planet's velocity need but be increased in the proportion of about 1,414 to 1,000, to lead to a similar result. And thus we see that the Sun's influence at the distances of the successive planets is limited to the control of bodies moving with a velocity bearing such a relation to the velocity of the respective planets. So that we have only to draw up a table of the distances and mean velocities of the planets, and to increase the latter quantities in the proportion of about 1,414 to 1,000, in order to have a representation of the gradual diminution of the Sun's influence at greater and greater distances. The table runs thus:—

Planet.	Mean distance in miles.	Mean velocity in miles per second.	Velocity increased as 1,414 to 1,000
Mercury	35,392,000	29·3	41·4
Venus	66,134,000	21·4	30·3
The Earth	91,430,000	18·3	25·9
Mars	139,311,000	14·7	20·8
The Asteroids . . .	250,000,000	11·0	15·5
Jupiter	475,692,000	8·0	11·3
Saturn	872,137,000	5·9	8·3
Uranus	1,753,869,000	4·2	5·9
Neptune	2,745,998,000	3·3	4·7

We see from this table, that if the three outermost

planets could only have imparted to them the velocity of the minor planets, they would be freed from their allegiance to the Sun, and pass away on hyperbolic orbits. But with the exception of Uranus, there is no planet which would be thus freed (absolutely) if it had imparted to it the velocity of the *next* inner one.*

But the above table has only been presented by way of introducing a more general law. What the table teaches us respecting special distances we can determine for all distances from the Sun, by a simple application of Kepler's third law and its results.

* It is a rather singular circumstance that the maximum velocity which the Sun can control at the distance of any one of the four outer planets should correspond so closely as it does with the actual velocity of the planet whose orbit lies next within. We see that if Neptune could have the velocity of Uranus, he would be almost wholly freed from his allegiance; Uranus would be just freed if he had the velocity of Saturn; Saturn would be almost wholly freed if he had the velocity of Jupiter; while Jupiter would be almost wholly freed if he had the velocity of those asteroids which travel at a mean distance. And there is a tendency, though less marked, to the same relation among the remaining planets. Remembering that the velocity a planet would require for freedom is that with which a body approaching on a parabolic orbit from an infinite distance would pass the mean distance of that planet, we have throughout the solar system a tendency (very marked among the outer members) to this remarkable relation, that the velocity with which a body approaching from infinity would cross the orbit of any planet should be the same as the actual velocity of the next inner planet. It need hardly be said perhaps that this relation directly results in the law to which Bode's law approximates for the outer planets. Thus, if the outermost planet had a distance D and a velocity V, the next inner planet would have a velocity $V\sqrt{2}$ corresponding to a mean distance $\frac{D}{2}$, and so on. But in the law of the duplication of the distances outwards there is no direct physical significance, whereas it is possible to conceive that the law as presented above —that is, regarded as associated with the velocities—may be associated also with the processes by which the solar system has reached its present condition.

Thus, suppose we wish to determine the maximum velocity which the Sun can control at a distance half that of the planet Mercury. Then the law that the cubes of the distances are as the squares of the periodic times shows us that the period of a planet at such a distance would be to Mercury's as 1 to the square root of 8 (or twice the square root of 2). Since then, the actual circuit of such a planet would be half that of Mercury, its velocity would exceed Mercury's in the proportion which the square root of 2 bears to 1,* or about 1,414 to 1,000. This would correspond to a velocity of 41·4 miles per second; and the greatest velocity the Sun could control at this distance would be obtained by increasing this velocity of 41·4 miles per second in the proportion of 1,414 to 1,000. It would therefore be 59·6. This shows how we can measure the Sun's controlling energy for any distance. But it also establishes a very important general relation. It appears that when we halve the distance, we have, in order to determine the velocity which the Sun can control, to increase the velocity at the greater distance in the proportion of about 1,414 to 1,000. And therefore when we take one-fourth of the distance, we must increase the velocity *twice* in this proportion. But this amounts to doubling the velocity, since this

* This illustrates the general law that if two planets have mean distances d and d' respectively, and mean velocities v and v' respectively, then

$$v : v' :: d'^{\frac{1}{2}} : d^{\frac{1}{2}}.$$

Clearly this is so, since the periodic times are as $d^{\frac{3}{2}}$ to $d'^{\frac{3}{2}}$, and the velocities, therefore, as $d \div d^{\frac{1}{2}}$ to $d' \div d'^{\frac{1}{2}}$.

proportion is that of the square root of 2 to unity. Hence we have this general rule, that the velocity which the Sun can control is doubled when the distance is reduced to one-fourth, and we can see at once how enormously the velocity must increase in the Sun's immediate neighbourhood.*

Thus, at a distance of 8,848,000 miles (one-fourth that of Mercury), the velocity the Sun can control, so as to compel a body to move in a closed orbit around him, is 82·8 miles per second; at a distance of 2,212,000 miles it is 165·6 miles per second; at a distance of 553,000 miles it is 331·2 miles per second. But this brings us very close to the Sun's surface—since it is from his centre all our distances are measured—and his radius is about 425,000 miles. The actual velocity at his surface—that is, the velocity which he could just control so as to compel a body to travel in a closed orbit just touching his surface—is easily obtained from the formula given in the preceding note. It is no less

* It need hardly be said that this result might have been obtained directly from a consideration of the law according to which gravity diminishes with distance. But apart from the fact that the mere dry reasoning by which the result would have been established would have had little interest to the general reader, the particular path which I have selected to follow has the advantage of introducing a number of independent relations, and of showing how the various matters dealt with bear upon each other and upon the general subject of the chapter.

The general law connecting distance with the velocity which the Sun can control is as follows:—Let d represent the Earth's mean distance; v her velocity at that distance in miles per second; D any other distance. Then the velocity which the Sun can control (so as to compel a body to travel in a closed orbit round him) at a distance D is

$$v\sqrt{\frac{2d}{D}}.$$

than 378·9 miles per second; and this is the least velocity with which a body must be projected from the Sun in order that it may never return to his globe again.

Thus the Sun, which at the distance of Neptune can control a velocity of but 4·7 miles per second, can control close by his own surface a velocity eighty times as great. At a distance four times as great as Neptune's, a velocity of 2·4 miles per second would suffice to enable a body to pass away to an infinite distance from the Sun. But even this velocity is enormous and almost inconceivable. If we seek to know in what regions the Sun could barely control velocities such as we are familiar with (by which I mean such velocities as the eye can appreciate, not velocities—as of cannon-balls or the like—which we speak of without appreciating), we shall find that such regions lie at enormous distances. A body moving at the rate of our swiftest express trains (say 60 miles per hour) would be compelled to travel in a closed orbit around the Sun, unless its distance from him were no less than 220,507,500,000,000 miles, a distance some ten or twelve times exceeding that of the star Alpha Centauri.*

We have seen that the Sun can control the motions

* At half this distance a planet travelling in a circle round the Sun would have a velocity equal to that of an express train; but if such a planet travelled in the plane of the ecliptic it would be unable to complete a circuit round the Sun on account of the disturbing influences of the star Alpha Centauri, which, being at only one-fifth of its distance from the Sun, would lie nearer to some parts of its circular orbit than the Sun does.

of a body travelling with a velocity of less than 378·9 miles per second close by his surface in such sort as to compel that body to travel in a closed orbit around him, always coming close by him at each return. That is the maximum velocity a body can have under such circumstances. It is also the greatest velocity a body can acquire in approaching the Sun, under his attraction alone, from an infinite distance and starting from rest. Now, the least velocity a body can have so as to travel close by the Sun is clearly that which would allow the body to travel in a complete circle around the Sun and close to his surface. We obtain this by simply reducing 378·9 in the proportion of 1,000 to 1,414; the required velocity is therefore 268·0 miles per second.* Such are the limits between which the velocities of all bodies travelling around the Sun so as just to graze his surface must necessarily lie.† Nor can any mass reach

* It may perhaps be necessary to point out that in a statement of this sort 268·0 is not strictly the same as 268. The former implies that the velocity lies between 268·05 and 267·95; the latter would imply that the velocities lie between 268·5 and 267·5.

† It may be interesting to inquire how far from the Sun a body would reach if projected vertically with a velocity which would just enable a body projected horizontally to complete the circuit of the Sun. The problem is exceedingly easy. Such a body would move as if travelling on an orbit having a mean distance equal to the solar radius. Conceiving the Sun's mass all collected at its centre, the body would, after passing to its greatest distance, return to the centre, round which it would circle with (for a moment) an infinite velocity, and so return to its aphelion. The aphelion distance, therefore, from the centre must be equal to the Sun's diameter, and the greatest distance attained from the Sun's real surface must be equal to the solar radius, or more than 425,000 miles. The rotation is not here considered. It would not, however, affect the distance attained by the body, though it would affect the real path by which that distance would be attained. We can

him from without, whether gathered directly out of space by his attraction, or reaching him after passing

in like manner determine the distance to which a body projected with any other velocity would reach. For we can determine by means of the formula in the note at page 63, what the mean distance corresponding to this velocity at such a distance from the Sun would be, and so (as before) we can determine the greatest height attained by the projectile. But there is a simple and convenient formula for this purpose, thus:— Let V be the velocity of projection, g the accelerating force of gravity at the Sun's surface, R the Sun's radius, and H the height attained by the body; then

$$V^2 = 2gH\frac{R}{R+H}.$$

If the reader prefer a formula in which the quantity g does not directly appear—so as to avoid the necessity of considering the value of terrestrial gravity (from which g is deduced) measured with reference to feet and seconds—we can easily get a convenient formula. Thus, we have

$$V^2 = 2gH\frac{R}{R+H},$$

and therefore from what has just been shown it follows that

$$(268{\cdot}0)^2 = 2gR\frac{R}{R+R}$$

(a mile being supposed to be the unit of length in the value of g and a second the unit of time). Therefore, dividing, we have

$$V^2 = 2(268{\cdot}0)^2\frac{H}{R+H}$$

or

$$V = 378{\cdot}9\sqrt{\frac{H}{R+H}}$$

(very convenient for finding V when H is given): and since $2(268{\cdot}0)^2$ is equal to 143,648, we have, for finding H when V is given

$$H = \frac{V^2R}{143{,}648 - V^2}.$$

It follows, see p. 72, that if V and v be the velocities with which two bodies falling from heights H and h respectively reach the surface of two globes having radii R and r, and mean densities Δ and δ, then

$$\frac{V}{v} = \frac{R\sqrt{\Delta}}{r\sqrt{\delta}}\sqrt{\frac{H}{R+H}}\Big/\sqrt{\frac{h}{r+h}} = \frac{R}{r}\sqrt{\frac{H.(r+h)\Delta}{h(R+H)\delta}}$$

through any number of forms of orbital motion due to the perturbing influences of the planets, with a velocity exceeding 378·9 miles per second, unless such a mass had already had motion communicated to it before the Sun began to act upon it. In like manner, no body can reach any given distance from the Sun with a velocity greater than that deduced, according to the above-discussed considerations, as the limiting velocity the Sun can control, unless that body had started on its journey towards the Sun with a velocity communicated beforehand, and by the influence of other attracting orbs. So that if it has been demonstrated, as many believe, that some of the meteors which reach our atmosphere travel through it with a velocity greater than 25·9 miles per second, superadded to the velocity with which the Earth meets them (which, according to the direction of impact, may have any value up to 18·5 miles per second), then that body comes to us with an inter-sidereal velocity, so to speak, in addition to such velocity as the Sun has communicated. For example, if a meteor penetrate our atmosphere with a velocity of 50 miles per second, coming full tilt against the Earth when she is near perihelion, than 5·6 miles per second at least of that velocity was communicated to the meteor by other orbs than our Sun.

It will naturally occur to the reader to inquire at

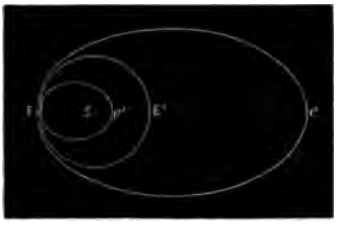
Also if $\frac{H}{h} = \frac{R}{r}$, then $\frac{V}{v} = \frac{R}{r}\sqrt{\frac{\Delta}{\delta}}$.

These formulæ are often useful.

It may be mentioned, however that with the assumed units (a mile for length and a second for time), $g = \cdot 160$.

this point what is the least velocity with which a meteor can travel when at the Earth's distance from the Sun. For clearly if meteors moving with velocities exceeding the greatest which the Sun's attractive energies can give, afford information of an extra-solar force to which such meteors have been subjected, so also meteors moving more slowly than any which could circle around the Sun would have to be regarded as indicating the action of some other force than solar attraction. Now, if we suppose a body travelling in the Earth's orbit, and as fast as the Earth, to have its velocity *increased*, we have seen that its orbit must needs have its greater axis increased, and will become parabolic if the increase of velocity be great enough. In fact, an increase of velocity makes the point where the increase takes place the perihelion of the new orbit. And obviously a decrease of velocity will make the point where the decrease takes place the aphelion of the new orbit. Thus, suppose the body is at E (fig. 20) and

FIG. 20.



travelling along the Earth's orbit E E′ about the Sun at S, when it suddenly receives an impulse onwards, then its new orbit will have such a shape as E *e*, E being

the perihelion. But if the body is checked when at E, the new orbit will be of such a figure as E e', the aphelion being at E. And the question we have to determine is, how great the reduction of velocity can be in order that the revolution of the body in an orbit may not be prevented. Clearly there is no limit to the reduction except that resulting from the size of the Sun. If the body can but pass clear round the Sun's globe, it will return to the point E, and (neglecting perturbations or resistance as it passes through the Sun's atmosphere) it will continue to circle in this way for ever. We have to determine what velocity at E is necessary for this purpose; and obviously, as a body moving more slowly at E would not pass clear of the Sun at S, so also, tracing its path back, it could not have arrived along a course clear of the Sun's globe; and the only interpretation of its motions would be *that it had been projected directly from the Sun's surface with sufficient velocity to reach the Earth.* It is not difficult to calculate the least velocity with which a body could travel when at the Earth's distance on an orbit just grazing the Sun's surface.* This velocity is

* There are many ways of attacking the problem. The best, perhaps, for our present purpose is the following:—

It is obvious that if a body be projected tangentially to the Sun's surface with such velocity as to just reach the Earth's orbit when at its greatest distance from the Sun, it would continue to revolve in such an orbit as we are inquiring into. Now, in order to determine the velocity of projection requisite for such a result, we may apply the formula in the preceding note, remembering that if a body projected vertically upwards from the Sun would reach to a height H, a body projected horizontally with the same velocity would travel in an orbit having a mean distance equal to $\frac{1}{2}(H \div R)$ where R is the Sun's radius; so that

about one-tenth of that with which the Earth moves in her orbit, or 1·85 mile per second; and no body can possibly reach the Earth with a smaller real velocity than this, unless actually projected from the Sun. A smaller *relative* velocity might very well be observed however. For example, a meteor travelling very little faster than the Earth might overtake her, and so seem to enter her atmosphere very slowly. And observa-

we require our body to be projected tangentially with such velocity as would be required to project a body vertically to a height equal to the Earth's mean distance D (say) from the Sun's *centre*, since the required orbit is to have a mean distance equal to $\frac{1}{2}(D+R)$. Hence, by the formula for v in the preceding note we have

$$v^2 = 2(268{\cdot}0)\,\frac{91{,}500{,}000}{91{,}925{,}000}\ \text{(approximately)};$$

whence we find the required velocity of tangential projection equal to 359 mile per second; and we note in passing that such a velocity as this would be required to project a body from the Sun to the Earth's distance. Now this is the perihelion velocity of our projectile when at a distance of 425,000 miles from the Sun's centre. In aphelion, its distance is 91,500,000 miles (approximately); hence from Kepler's first law its velocity there will be

$$359\,\frac{425{,}000}{91{,}500{,}000}\ \text{miles per second,}$$

or about 1·85 mile per second, which is almost exactly one-tenth of the Earth's velocity in her orbit, but considerably exceeds the Moon's. This velocity would, however, be somewhat increased by the Earth's attraction. It is to be noticed that the Moon's velocity in her orbit being about $\frac{2}{3}$ths of a mile per second, the greatest velocity the Earth can control at the Moon's distance is but about $\frac{17}{20}$ths of a mile per second; so that no body moving to the Earth's neighbourhood under the influence of the Sun's attraction can by any possibility be compelled by the Earth to travel in an orbit around her unless it comes much nearer than the Moon. The maximum velocity which the Earth can control close by her surface is, however, about seven miles per second; so that bodies having the aphelion of their orbit nearly at the Earth's distance and the perihelion close to the Sun's surface, might, if they happened to come close by the Earth, be compelled to circuit in an orbit round her.

tions seeming to indicate that the real velocity of a meteor was less than 1·85 mile per second, would need to be very strongly confirmed before they would be accepted by astronomers; since to reach the Earth *a body must be projected from the Sun with a velocity of about* 360 *miles per second.*

Here, having already passed the limits I had proposed to myself in dealing with the subject of this chapter, I draw it somewhat regretfully to a close. It would have been easy to extend the chapter so as to have occupied a volume twice the size of the present, and yet to have discussed no well-worn facts. The whole subject of the Sun's rule over the space surrounding him has remained in a sense almost untouched, until lately its significance began to be recognised by Mayer, Thomson, Waterston, and others. Even these, however, have dealt rather with the limits of activity possessed by bodies close to the Sun's surface than with the velocities which measure his influence at greater distances. For my own part, I find a wonderful interest in the ideas suggested by the Sun's activity throughout the whole range of his wide domain. The velocities to which I have referred in this chapter as those which the Sun can control are also those which he can generate. We have to think of him, therefore, as capable of drawing towards himself all such cosmical matter as comes under his exclusive attraction either by leaving the domain of some other star, or on account of his own motion through space. In so drawing cosmical materials towards himself, he imparts to them

velocities such as we have been considering in the present chapter. The vaster the distances from which they come, the greater the velocities he imparts to them. As they sweep onward in their course they are subject to the influences of the planets—the patrols of the solar system—and under such disturbing influences large numbers must be compelled to follow, either temporarily or permanently, true orbits (that is, closed curves) around the Sun. But the majority of such visitants, whether comets or meteors, must return to the sidereal depths after once paying their respects —in the full rush of their perihelion swoop—to the giant ruler of our system. It seems probable that in this continual rush of matter, this continual interchange of attendants on suns and stars, we may recognise the progress of processes exercising a most important influence on the welfare of planetary systems. It is still more probable that the bodies which are finally drawn into the solar domain perform highly important functions in the economy of our own particular planetary scheme. But unless I mistake, the real significance of the considerations we have dealt with in the present chapter lies in their bearing on the past history of the solar system. The rush of matter which we now recognise affords perhaps but the faintest indication of the amazing conflicts in which our system had its birth. Tracing back the history of that system, we seem to recognise a time when the Sun's supremacy was still incomplete, when the planets struggled with him for the continually inrushing materials from which his sub-

stance as well as theirs was to be recruited. We can see him by the mighty energy of his attraction clearing a wide space around him of all save such relatively tiny orbs as Venus and the Earth, Mars, Mercury, and the asteroids. With more distant planets the struggle was less unequal. The masses which flowed in towards the centre of the scheme swept with comparatively slow motion past its outer bounds, so that the subordinate centres there forming were able to grasp a goodly proportion of material to increase their own mass or to form subordinate systems around them. And so the giant planets, Jupiter and Saturn, Uranus and distant Neptune, grew to their present dimensions; and became records at once of the Sun's might as a ruler—for without his overruling attraction the material which formed these planets would never have approached the system—and of the richness of the chaos of matter from which his bulk and theirs were alike evolved. Nor is the consideration without a mysterious attraction, that in thus looking back at the past history of our system we have passed after all but a step towards that primal state whence the conflict of matter arose. We are looking into a vast abysm, and as we look we fancy we recognise strange movements, and signs as if the depths were shaping themselves into definite forms. But in truth those movements show only the vastness of the abysm, those depths speak to us of far mightier depths within which they are taking shape. 'Lo! these are but a portion of His ways; they utter but a whisper of His glory.'

CHAPTER III.

ANALYSING SUNLIGHT.

THE researches of telescopists have revealed many important facts respecting the Sun's constitution. Studied thoughtfully, these researches enable us to answer many questions which at first sight seem to require more powerful modes of inquiry. But, undoubtedly, the science of solar physics is too vast and too difficult to be satisfactorily treated by telescopic research alone. The condition of the Sun is so different from that of any bodies we can experiment on, that his mere aspect —and the telescope can show us nothing more—is insufficient to tell us what his constitution may be. The picture of the Sun presented by the most powerful telescope resembles a book, full of meaning indeed, but written in an unknown language. The spectroscope is the means by which that unknown language has been in part interpreted.

Let us consider what spectroscopic analysis really is. It is scarcely possible to treat of any astronomical subject in the present day without describing the most powerful of all instruments of astronomical research; but in the case of the Sun it would be hopeless to

attempt such a course. Spectroscopic analysis forms the very basis of all our ideas respecting solar physics. We must thoroughly understand the mode in which it teaches, the meaning of its teaching, and the extent to which its teaching may be relied on, otherwise our views will be vague and unsatisfactory, depending rather on the statements of others, than on any clear apprehension of their truth on our own part.

For this reason I shall spare no pains to make the explanation of spectroscopic analysis which follows as simple, and, at the same time, as complete as possible. There is, in reality, nothing difficult in the subject; but it cannot be denied that, considering its simplicity, it is not nearly so well or so widely understood as it might be,—a circumstance the more to be regretted because the whole history of recent scientific researches is a sealed book to those who do not clearly recognise the nature of the instrument by which those researches have been effected.

Newton was the first who showed that white light is a compound of light of many different colours.* He proved this by his investigation of an experiment of Grimaldi's—illustrated in fig. 21. Here A B represents the course of a pencil of solar light† passing through

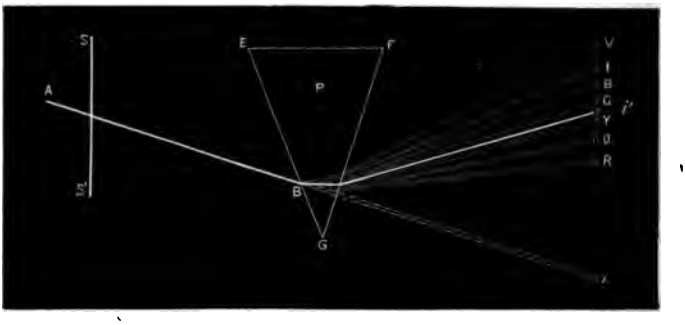
* Grimaldi was, however, the first who discovered the effect of passing sun-light through a prism. See his *Physico-mathesis de lumine*, prop. XXX. et seq.)

† I have purposely modified Newton's figure, because many misunderstand a figure in which the pencil A B is shown with its proper divergency. They confuse the dispersing effect of the prism with the optical effects produced on a diverging pencil of *pure* light. In considering the spectroscope as actually employed we may neglect the effect

a circular aperture in a screen S S′. The prism P is so placed as to intercept the light. It will be well to consider the prism as placed with the base E F uppermost and horizontal.*

Now, if the prism were removed, the light would fall at *i* and make a small elliptical image there. And if the solar light were simple, instead of being composed of rays of many different colours, it would follow such

FIG. 21.



a course as is indicated by the bent bright line, and form a small elliptical image at *i′*—this image being white like that at *i*, and resembling the latter image in shape.†

But instead of this, Newton found, as Grimaldi

of divergency altogether; because an arrangement is employed for parallelising the pencil analysed by the prism.

* E G is supposed equal to G F, so that these lines are equally inclined to the vertical.

† It would indeed be absolutely identical in shape with the image at *i* if the exact course indicated in the figure were followed; but if the single image fell at V or R this would not be the case. It need hardly be remarked that for pure light the course of the beam would depend on the refracting angle G of the prism.

had found before him, that a streak of light was formed as V R (V R is exaggerated in length), the streak being violet at the highest point and thence changing through indigo, blue, green, yellow, and orange, to red at the lowest point. Neither above nor below was the streak well defined, but passed gradually into darkness. At the sides, however, the streak was well defined, and in breadth equal to the horizontal breadth of the figure at *i*. It thus formed a rainbow-tinted streak or ribbon.

It appears from this experiment that light consists of rays of all the colours of the rainbow, that the violet rays are the most bent by the action of a prism, the red rays least, the others in the order named above. This happens with prisms of all refracting angles and of whatever substance. Hence the rays forming the violet part of the spectrum are often called—without further description—the most refrangible rays, while the red rays are called in the same way the least refrangible rays. This mode of speaking, and the expressions arising from it, should be carefully noted.

Now, the streak of light seen by Newton showed no breach of continuity. Newton appears to have suspected the possibility that by a change in the conditions of his experiment the streak would show gaps. In other words, he suspected that light of all degrees of refrangibility, between the light which forms the extreme violet and the extreme red of the streak, may not be present in the solar beam. But he did not succeed in proving this, though he employed apertures

of different shape through which to admit the light. It is clear that if there were simple violet light, and simple indigo light, and so on in the solar beam, a succession of small coloured images would be formed as shown in the figure at V, I, &c., and between these images dark spaces would be seen. Newton's experiments led him to the conclusion that an infinite number of images, shifting by indefinite gradations from V to R, exist along the streak, and so cause the colour to vary insensibly from violet to red as observed in his first experiment.

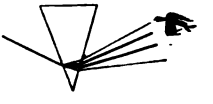
Wollaston was the first who succeeded in showing that there are gaps in the spectral streak.

It is clear that the circular aperture in the above experiment is not suitable for determining whether rays of all degrees of refrangibility are included in a beam of solar light. In fig. 21 an image of the aperture is represented at V, another at I, another at B, and so on; though of course the spectrum in Newton's experiment showed no such separate images. Now, it is perfectly obvious that if instead of the seven images represented in the figure there were twenty or thirty along the spectrum V R, there would be no means of knowing that the spectrum was made up of these twenty or thirty distinct images, for they would overlap, and so show a continuous streak of light. Wollaston found that when, instead of a circular, triangular, or oblong aperture, a very narrow slit is employed, light of certain degrees of refrangibility is absent from the solar beam. He admitted the light

through a narrow slit (parallel to the edges of the prism). With this arrangement the spectrum seen by Wollaston was not continuous, but crossed by two dark lines parallel to the slit, or, in other words, at right angles to the length of the spectrum.

These two lines—two *gaps* in the solar spectrum—proved that light of two definite orders of refrangibility is absent from the solar beam. Fig 22 shows how

FIG. 22.



the light, after passing through the prism, had become divided into three parts, with spaces between them along which no light travelled. It is quite obvious that the existence of these gaps can be recognized without allowing a spectrum to be formed on a screen, as in Newton's experiment—simply by placing the eye as shown in fig. 22. It was in this way that Wollaston observed the two gaps. It may be remarked in passing that this mode of viewing the spectrum bears the same relation to Newton's plan that observation of the Sun with the naked eye bears to observation of the Sun's image received upon a screen.

Dr. Wollaston did not pursue the inquiry further. Nor need we greatly wonder at this, if we rightly consider the matter. We now know, indeed, that in the two dark spaces on the spectrum of Wollaston there lay the germ of the most wonderful discoveries man

has yet made. We know that had he persisted in the inquiry his name would have been associated through all time, as that of Fraunhofer will undoubtedly be, with the very language of the new analysis. But it must be admitted that Wollaston had little reason for expecting any very remarkable results from the study of a peculiarity which seemed quite as likely to depend on the nature of the glass of which the prism was made as upon any inherent property of solar light. And even supposing that the gaps were due to some peculiarity of solar light, who could suspect that that peculiarity, when traced to its source, would be so full of meaning as to reveal to us the very constitution of the solar orb?

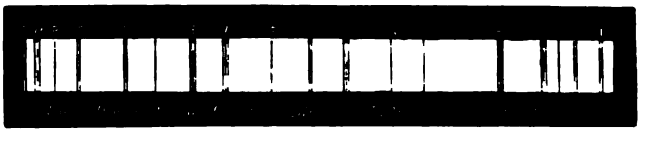
It is to the unwearying patience with which Fraunhofer—like so many others of his countrymen—was willing to work day after day at what seemed a most unpromising subject, that the world owes the first complete recognition of the characteristic peculiarities of the solar spectrum. Wollaston had observed the spectrum directly, with the unaided eye. Fraunhofer improved on this plan by employing a telescope.*

* It is well to notice that all the modes of viewing the Sun are available for viewing the solar spectrum. A prismatic spectrum is simply a series of images of a luminous object formed by rays of different refrangibilities. Where the luminous object is a line of light—as where light is received through a fine slit—the solar spectrum is in reality made up of an infinite number of lines of light at right angles to its length. It is because light of certain definite orders of refrangibility is wanting that images of the line are wanting at certain definite parts of the spectrum,—that, in other words, dark lines are seen. The clear recognition of this fact will prevent much misapprehension.

Dr. Wollaston had seen but two gaps in the solar spectrum; Fraunhofer, in 1814, saw and mapped no less than 576 lines. The positions of the chief lines, seen by Fraunhofer, are indicated in fig. 23, and as reference is continually made to the lettered lines, it is well that a student should carefully study their sequence and position.

A is a well-marked line close to the limits of visibility at the red end of the spectrum. B is a well-defined red line of sensible breadth.* Between A and

FIG. 23.



B is a band of several lines called *a*. C is a dark and very well-marked line. Between B and C Fraunhofer counted nine fine lines; between C and D about thirty. D consists of two strong lines close together. Between D and E Fraunhofer counted eighty-four lines. E is a band of several lines, the middle line of the set being stronger than the rest. At *b* are three strong lines, the two farthest from E being close together. Between E and *b* Fraunhofer counted twenty-four lines, and

* It will be understood that I am here describing the lines as seen by Fraunhofer, and as they would appear with similar spectroscopic power to that employed by him. With greater power many single lines are resolved into several, and many new lines make their appearance. For example, D is described above as consisting of two strong lines with an extremely small interval. With a powerful spectroscope numerous lines are seen between these two strong lines.

between *b* and F more than fifty. F, G, and H are strong lines. Between F and G, and between G and H, Fraunhofer counted 185 and 190 lines respectively; and he found many lines also between H and I—at the violet end of the spectrum.

Fig. 23 shows the colours of those parts of the spectrum in which the several lines occur. The reader will do well to bear in mind the position of the several lines, as thus, by an easily remembered relation, he will find himself enabled to interpret readily the accounts of spectroscopic researches into various subjects of inquiry. Let him remember, then, that A, B, and C are in the red portion of the spectrum; D in the orange-yellow; E in the yellowish-green; F in the greenish-blue; G in the indigo; and H in the violet.

Fraunhofer's contributions to the science of spectroscopic analysis did not conclude, however, with the recognition and mapping of these lines. Having first convinced himself that the same lines were seen in the solar spectrum, of whatever substance the prism was formed, he proceeded to study the spectra formed by light from other sources.

He first examined solar light received indirectly by reflection or otherwise—as from the clouds, the sky, the Moon or planets, and so on; and he found in the spectrum of such light the same lines which he had seen in the spectrum of direct solar light. He studied the spectrum of the Sun when that orb is near the horizon, and he found that under such circumstances the violet end of the spectrum disappears and

a number of lines make their appearance in the remainder of the spectrum.

Fraunhofer found that the spectra of the fixed stars exhibit dark lines resembling those in the Sun; but none of the stars whose light he examined had a spectrum exactly the same as the Sun's. Some lines of the solar spectrum he found wanting in star spectra, while other lines were not seen with the same relative distinctness as in the spectrum of the Sun. On the other hand, he found several new lines in star spectra. No two stars appeared to have the same spectrum.

An important conclusion followed, as Fraunhofer pointed out, from this observation. If the dark lines in the solar spectrum were caused by an absorptive action exercised by our own atmosphere, it would follow that the same lines ought to be seen in the spectra of the fixed stars. The contrary being the case, Fraunhofer held it to be a demonstrated fact, that the dark lines are due to some property inherent in the light itself which the Sun and the fixed stars severally emit.

One more observation of Fraunhofer's, and I pass on to later researches. He found that when the flame of a candle or lamp is the source of light, the spectrum is not crossed by dark lines, but a bright double line is seen in the exact place occupied by the double dark line D of the solar spectrum. To prevent misconception, it is necessary, however, to mention that light from an incandescent substance—and the flame of a candle is such light—exhibits a continuous spectrum.

The double bright line seen by Fraunhofer was due to the presence of the almost ubiquitous element, sodium, in the flame, as will be explained further on.

If we carefully weigh the results obtained by Fraunhofer, it will appear that spectroscopic analysis owes very much to his researches. It may be questioned, indeed, whether, but for his patience and perseverance, the attention of scientific men might not have been turned away, at least for many years, from a subject of inquiry which seemed when he began his labours to be rather curious than important.

Let us next inquire into the spectra given by different terrestrial sources of light.

An incandescent solid or fluid—or, to speak more correctly,* a solid or fluid *glowing* with intensity of heat—gives a continuous spectrum. But the nature of the spectrum varies with the heat of the source. If a piece of metal, for instance, be gradually heated till it is at a white heat, only the red part of the spectrum will at first be visible; then the orange part will show, then the yellow, and so on, until at length the whole range of the spectrum will be seen, from the extreme red to the extreme violet.

* The term incandescent is not properly applicable to any source of light which is not actually white. Many spectroscopists indeed go further, and say that no luminous body ought to be described as incandescent unless its spectrum extends without dark lines or gaps of any sort from the extreme limit of visibility at the red end to the extreme limit of visibility at the violet end of the solar spectrum. Without insisting on this limitation, it certainly does seem well to point out that the term incandescent is not properly applicable to solids or fluids glowing with light belonging to the red end of the spectrum, nor to vapours glowing with light of a well-marked colour.

But the question will at once suggest itself at this point,—What are the limits of the spectrum? When we were considering the solar spectrum, we might have inferred that the limits of visibility are the true limits of the space to which the solar action extends. But now that we have seen a spectrum growing with the growing heat of the source of light, we are naturally led to inquire whether there are limits to this growth. And, again, the spectrum of a gradually heated body begins at the red end so far as our vision is concerned; but is that its true beginning?

To both these questions an answer has been given. It occurred to several physicists during the latter half of the last century to inquire whether the heat which undoubtedly accompanies the light forming the spectrum corresponds in reality with the light, in so far that where the light is strongest the heat is strongest, and *vice versâ*, while where light fails totally there heat also fails. They found that no such correspondence exists. So far as the visible spectrum is concerned, the greatest heat is not received where the spectrum is brightest, but at the red end. Sir W. Herschel, however, at the beginning of the present century, found that the maximum of heat comes from *beyond* the red end of the spectrum; while, yet farther beyond the red end, heat continues to be received for a distance whose extreme limit has not yet been determined.

Heat, however, is not received beyond the violet end of the spectrum, nor even completely up to the limits of visibility in that direction.

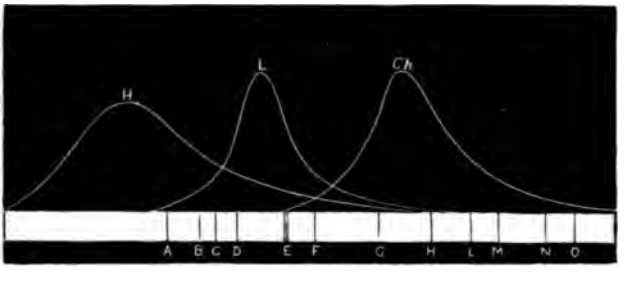
Yet we now know that in this direction, also, the limits of visibility are not to be regarded as the limits of the solar action. Besides the heat and light which are transmitted through the prism, there is another form of action whose effects are as distinct from heat and light as heat and light are from each other. We know that the solar rays, besides illuminating and heating substances on which they fall, produce changes in the appearance and constitution of many substances. To take a familiar example: the solar rays falling on the skin not only warm it, and so affect the sense of touch; not only illuminate it, and so affect the sense of sight; but *tan* it,—an effect which is not directly cognisable by any sense that we possess,* though in-

* It need hardly be said, perhaps, that the reference here to the senses is not introduced as bearing in any way on the subject of spectroscopic analysis, but as affording a distinction of a popular kind between the three forms of solar action. It is worthy of notice, however, that we have a sense by which the action of the longer light-waves corresponding to the red end and the parts beyond the red end of the spectrum is recognised by us, and another sense enabling us to recognise the action of the medium waves corresponding to the yellow part of the spectrum, and in gradually diminishing degree the waves corresponding to parts up to the red end on one side and the violet end on the other side: but we have no sense enabling us to recognise directly the action of the shorter waves corresponding to parts of the spectrum beyond the violet end. Is it not conceivable that some creatures, even among terrestrial beings, may possess a sense enabling them to recognise the action of these short waves, and that such a sense may give them powers as distinct from the powers we possess in virtue of the senses of touch and of sight, as the sense of sight is distinct from the sense of touch? A man born blind may not be more incapable of conceiving the nature of the sense of sight and of the powers it confers upon those who possess it, than those who have all the five senses are of the powers which may be actually possessed by creatures having organs suited to appreciate the action of the shorter light-waves.

directly sensible both to sense and touch. The real nature of this action is not altogether understood (which may be remarked also respecting heat and light), but the observed result is a modification of the chemical condition of the substances acted upon.

Now this particular mode of action—actinic action, as it has been called *—is not exerted most powerfully where the spectrum is brightest, but near the violet extremity—between the lines G and H. Nor is this mode of action limited to the visible spectrum; but precisely as heat falls beyond the red extremity of the

Fig. 24



spectrum, so actinic effect is produced beyond the violet extremity.

In fig. 24 the relation between the several kinds of action is exhibited by the curves marked H, L, and *Ch* respectively, the height of these curves vertically above the spectrum indicating the relative intensity of the heat, light, and chemical activity of the portion of the spectrum immediately below. Thus, beyond the red

* From a Greek word signifying a ray. The term is not very well chosen.

end, the heat curve is seen to acquire its greatest height, indicating that the maximum heating effect is exerted there. The light-curve reaches its greatest height above the part of the spectrum between D and E, in the yellow portion, that is, of the spectrum. The heat-curve reaches the base-line close by G, while the light-curve extends somewhat beyond H. At E the actinic curve rises above the base-line, reaching its greatest height above G and H, and thence passing down to the base-line far to the right of the violet end.

It appears, then, that from the extreme heat end of the spectrum to near A there is heat only; from near A to about E we have heat and light combined; from E to G, all three forms of action—light, heat, and actinism—are present.* From about G to a little beyond H, light and actinism (though very little of the former) are exerted, while from a little beyond H to the extreme actinic end of the spectrum there is actinism alone.

It may be well, however, to warn the reader against the error sometimes made, of assuming that where two forms of action are present they are separable (or can be conceived to be separated) from each other. For example, it would be incorrect to say that near D a small proportion of heat rays, and a larger proportion of light rays, fall on the spectrum. The correct statement is that those rays (or rather light-waves) which reach this part of the spectrum are capable of exciting

* It is worthy of notice how small the amount of all three forms of action becomes near the F line of the spectrum.

heat to a certain extent, and light to a somewhat greater extent.

We see, then, that in addition to what spectroscopic analysis has already taught us, we must add this interesting fact, that the three forms of action which sun-light is capable of exerting are associated with different parts of the spectrum—heat with the red end and parts beyond; chemical action with the violet end and parts beyond that; and, finally, light more particularly with the yellow part of the spectrum, though of course, as the very term visible spectrum implies, more or less light is received from all parts of the coloured spectrum.

And the intimate real association which exists between the three forms of action is shown by nothing more distinctly than by this, that when a solid or fluid body is gradually raised to a white heat, all the forms of action are generated:—first, heat; then heat and light; then heat, and light, and actinism—as the formation of the continuous spectrum of such bodies serves abundantly to prove.

Only two or three solid substances give a non-continuous spectrum when heated. So that it has come to be regarded, as a characteristic peculiarity of incandescent solids and fluids, that they present a rainbow-tinted streak of light crossed by no dark lines or gaps.

With glowing vapours the case is altogether different. Although there are exceptions to the rule, it may be stated as a general characteristic of the spectra of such vapours that they consist of coloured lines or bands.

Sir David Brewster, Sir John Herschel, and Talbot, were among the first who examined such spectra. In 1822 Sir John Herschel called attention to the importance of the study of the lines and bands seen in the spectra of the vapours of different elements. 'The pure earths,' he said, 'when violently heated, yield from their surfaces lights of extraordinary splendour, which when examined by prismatic analysis are found to possess the peculiar definite rays in excess which characterise the tints of the flames coloured by them; so that there can be no doubt that these tints arise from the molecules of the colouring matter reduced to vapour, and held in a state of violent ignition.'

It would be interesting to trace the history of those laborious researches by which men of science—Herschel, Brewster, Tyndall, the Millers, Huggins, Gladstone, Frankland, Plücker, Hittorf, and many others—have determined the spectra of gases and vapours by various methods and under various conditions. But as in this treatise I feel bound to deal only with those parts of the history of spectroscopic analysis which are associated with the study of the Sun, or which serve to elucidate solar physics, I must here content myself with indicating results without describing the processes by which they were obtained, or assigning to each of the eminent men above-named, and to their fellow-workers their exact share in the noble series of labours referred to.

At an early stage of the inquiry, though at the time the phenomenon was not correctly interpreted, a means

was found of obtaining the spectra of the vapours of elements which cannot be vaporised by ordinary methods. It was noticed that the electric spark has a spectrum consisting of bright lines. But, unlike all the sources of light whose spectra have hitherto been considered, the electric spark yields a variable spectrum. When the electric discharge takes place between conductors of the same nature, and through any given and unchanged medium, the same spectrum is always seen; but when either condition is departed from, a different spectrum is obtained.

It was presently recognised that the spectrum obtained from the electric spark is twofold in its nature. It includes the spectrum of the gas or vapour through which the discharge takes place, and also the spectrum of the vaporised substance of the conductors.

Here, then, was a ready means of determining the spectra of the metallic and other elements * not easily volatilised in other ways, as also of such gases as nitrogen and oxygen; while conditions of pressure, temperature, and the like, could be introduced, which would be wholly impracticable if the method of vaporising elements in the flame of an ordinary lamp had alone been available.

In this way a large number of elements had their spectra assigned to them, while in several instances physicists began to recognise peculiarities in the

* It seems unnecessary to speak of the spectra *of the vapours* of such and such elements, since in reality it is only as vapours that iron, sodium, and the rest have characteristic spectra at all.

spectrum of the same element when examined under different conditions.

But an important series of researches yet remains to be considered. Hitherto we have dealt with the actual spectrum of luminous objects; we have now to inquire into the effects produced on these spectra, when the light which forms them is allowed to pass through absorbing media. We owe chiefly to Sir David Brewster the initiation of this branch of research,—though in this, as in so many other departments of spectroscopic inquiry, we find a host of distinguished physicists joining in the work. Brewster found that when ordinary solar light is transmitted through the thick vapour of nitrous gas, a number of new dark lines are seen, parallel to the Fraunhofer lines, and congregated in a remarkable degree towards the violet end of the spectrum. He further proved that these lines were seen whatever the source of light might be. Professors Miller and Daniel made further researches into the effects of vapours in causing dark lines to appear in the spectrum of solar or white light. Some of Professor Miller's results are worth quoting, because they show how closely a physicist may approach a great discovery without actually effecting it. 'First,' he says, 'colourless gases in no case give additional lines, or lines differing from those of Fraunhofer. Secondly: the mere presence of colour is not a security that new lines will be produced; for instance, of two vapours undistinguishable by the eye, one, *bromine*, gives a great number of new lines, while the other, *chloride of*

tungsten, exhibits none. Thirdly: the position of the new lines has no connection with the colour of the gas; with *green perchloride of manganese*, the new lines abound in the green of the spectrum; with *red nitrous acid* they increase in number and density as we approach the spectrum's blue extremity.' Some of these results, rightly understood, contain the germ of the great discovery afterwards effected by Kirchhoff; since in some of the cases actually experimented on by Professor Miller, the absence of *new* lines meant simply that the absorption lines, corresponding to the element he was dealing with, were coincident with some of the Fraunhofer lines. Others, however, approached the solution of the great problem even more nearly, since they actually touched on the principle of the reversal of the spectral lines, which affords the explanation of the coincidences detected but unnoticed by Professor Miller. 'None of these distinguished men,' says Professor Tyndall, 'betrayed the least knowledge of the connection between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject was Ångström. In a paper translated from Poggendorff's "Annalen" by myself, and published in the "Philosophical Magazine" for 1855, he indicates that the rays which a body receives are precisely those which it can emit when rendered luminous. In another place he speaks of one of his spectra giving the general impression of reversal of the solar spectrum. Foucault, Stokes, Thomson, and Stewart, have all been very

close to the discovery; and for my own part the examination of the radiation and absorption of heat by gases and vapours would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation.'

At the very moment, however, when the great secret was about to be revealed, an eminent physicist wrote thus: In quitting the mere phenomena of luminous spectra, and rising to the inquiry as to their causes, we enter a more arduous course. The phenomena defying, as we have seen, all attempts hitherto to reduce them within empirical laws, no complete explanation or theory of them is possible. All that theory can be expected to do is this—it may explain how dark lines of any sort may arise within the spectrum.'

But theory succeeded in doing very much more than was thus anticipated from her.

It had been noticed by Fraunhofer that the two orange-coloured lines close together which form the spectrum of the glowing vapour of sodium, coincide exactly in position with two dark lines in the orange-coloured part of the solar spectrum—the double D line in fig. 23. Kirchhoff, having a spectroscope of great dispersive power,* determined to inquire whether this

* Its prismatic battery contained four flint-glass prisms, not fixed or clamped in any way, but standing freely on four little pedestals. The whole arrangement seems singularly ineffective by comparison with the spectroscopes now in use; for example, if we compare Kirchhoff's battery with the battery of prisms in the fine spectroscope made by

coincidence was exact. 'In order to test in the most direct manner possible,' he says, 'the frequently asserted fact of the coincidence of the sodium lines with the lines D, I obtained a tolerably bright solar spectrum, and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones. The flame of a Bunsen's lamp threw the bright sodium lines upon the solar spectrum with unexpected brilliancy. In order to find out the extent to which the intensity of the solar spectrum could be increased without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame, and, to my astonishment, I saw that the dark lines D appeared with an extraordinary degree of clearness.'

Let the full force of this result be recognised before we proceed to consider the method by which Kirchhoff certified himself as to its exactness. He had shining in through the slit two kinds of light—sunlight and the light of the sodium flame. The sunlight alone would have given the ordinary solar spectrum with the D lines of a certain degree of darkness. The sodium flame alone would have given two bright lines just where the dark D lines of the solar spectrum appear. What he expected was, naturally, that the bright lines of the sodium spectrum would at least partially reduce the darkness of the corresponding D lines, even if they

Browning for Mr. Gassiot, the former seems almost ridiculous in its imperfectness. 'A cough or a sneeze,' it has been well remarked, 'would set that whole battery in disarray, with which, nevertheless, Kirchhoff solved the secret of the solar spectrum.'

did not altogether cause these dark lines to disappear, or to be replaced by bright lines. But these lines actually appeared darker. It was precisely as though an experimenter were to cast a beam of light exactly upon a shadow, and to see the shadow actually intensified instead of the reverse.

Kirchhoff proceeded to test this astonishing result. 'I exchanged the sunlight for the Drummond's or oxy-hydrogen lime-light, which, like that of all incandescent solid or liquid bodies, gives a spectrum containing no dark lines. When this light was allowed to fall through a suitable flame coloured by common salt, dark lines were seen in the spectrum in the position of the sodium lines. The same phenomenon was observed if, instead of the incandescent lime, a platinum wire was used, which, being heated in a flame, was brought to a temperature near its melting point by passing an electric current through it.'

These experiments were, if possible, even more striking than the former; for now Kirchhoff had two lights which seemed to produce darkness. The electric light alone covered with its continuous spectrum the place where the sodium lines appear in the solar spectrum; the sodium light alone lit up this very part of the spectrum. When the two lights were both shining one would have expected the result to be an increased brightness in this part; instead of which there actually resulted relative darkness.

The observed fact is in itself important: its interpretation involves one of the most important facts ever

discovered by man. It is well to distinguish between the two.

The observed fact is that the sodium flame, which emits rays of a certain order of refrangibility—that is, rays which, if passed through a prism, will follow a certain path—has the power of absorbing rays of precisely the same order. The interpretation of the fact is founded on the existence of a law which is thus worded by Professor Roscoe: 'Every substance which emits *at a given temperature* certain kinds of light must possess the power *at that same temperature* of absorbing the same kinds of light.'*

* This is merely a corollary from a more general law, according to which the same relation holds between the powers which substances possess of emitting and absorbing heat-waves as well as light-waves and actinic-waves. The law called the theory of exchanges was first enunciated for heat by Prevost of Geneva, and has since been established for heat and light by the researches of Prevostaye, Dessains, Stewart, and Kirchhoff. So far as the application of the theory to light is concerned, it must be admitted that there are still many difficulties in the way of its complete acceptance. These difficulties somewhat importantly affect our conclusions where we are considering the application of the theory in mode and measure to spectroscopic researches, though, so far as the general results here chiefly considered are concerned, they need not greatly trouble us.

I may refer my reader to Dr. Stewart's *Elementary Treatise on Heat* for a very interesting examination of the subject and the demonstration of the fundamental principles involved in the law. But there is one point in the course of Dr. Stewart's reasoning which involves an error in an optical subject of considerable importance. It is necessary for the demonstration that the course of rays (light-rays or heat-rays) not falling quite perpendicularly, or rather not strictly parallel to each other. should be considered, and Dr. Stewart removes the difficulty for small beams of light or heat, by considering the size of the source of light. He says, 'Just as a line is in reality always part of the boundary of a solid, so a ray is always in reality part of the boundary of a beam or pencil of light. We may satisfy ourselves that this is the case in nature

Kirchhoff experimented on other elements. He found that a flame coloured by potassium causes dark lines to appear on the continuous spectrum of the lime-light, and that these lines appear precisely where bright lines are seen when the spectrum of that coloured flame is viewed alone. The same was proved by Kirchhoff and Bunsen for the lines of lithium, calcium, strontium, and barium; while it has been shown to hold for other elements by Dr. Miller and others.

But now that the general law was established, important results respecting solar physics were established along with it. Since Kirchhoff had proved that when the electric light shines through a sodium flame, the sodium lines appear as dark lines across the continuous spectrum of the electric light, what was more obvious than

by considering the light which reaches the eye from a star or other object apparently very small; this would seem to be the nearest approach to a geometrical line of light, whereas since a star has a certain real, though very minute, angular diameter, the light from it is in reality a converging pencil, although no doubt the angle of convergence is very small.' On account of the importance of the optical considerations in question, I may be permitted to correct what is undoubtedly an erroneous statement. The difficulty must be got over by considering the size of the object on which light or heat rays fall, not by considering the size of the source of light. There is no such thing in nature as a converging pencil of light proceeding directly from a luminous object. All real pencils, whether of light or of heat, are originally diverging; they diverge from every point of the self-luminous or heat-giving body, and the angle of their divergence is real—however minute it may be—so long as the object which receives light or heat has real dimensions, however minute. This consideration does not affect the conclusions deduced by Dr. Stewart, since undoubtedly the diverging pencils of light or heat, from a source of considerable dimensions, form a converging beam, when we consider them with reference to a smaller object on which they fall.

the conclusion that sunlight also, which shows these same dark lines, must, before reaching us, have passed through the vapour of sodium? Either then, in our own atmosphere, or in the atmosphere of the Sun, this familiar metal sodium, the basis of such commonplace substances as salt and soda, must exist in quantities sufficient to cause the observed absorption-lines.

But more than this. Those countless other lines which cross the solar spectrum must each indicate some process of absorption exerted by vapours in our atmosphere or his. Since the presence of sodium has thus been demonstrated, why may not the presence of other elements be in like manner rendered apparent?

Kirchhoff did not long delay the inquiry thus suggested. He compared the spectrum of iron (or, to use the accepted mode of expression, the spectrum of the luminous vapour of iron) with the solar spectrum. Now, the spectrum of iron as known to Kirchhoff contained some sixty bright lines.* There was a great difference, then, between this spectrum and that of sodium. Yet, to his astonishment, Kirchhoff found that precisely as the two sodium lines agree with the D lines of the sodium spectrum, so every one of the iron lines had its counterpart in the solar spectrum.† Line for line, and not only so, but strong

* Many more have since been discovered; in fact, there are now more than 450 recognised iron lines.

† It is hardly necessary to note, perhaps, that Kirchhoff could not establish this result in the same way as the former, because he could not cause sunlight to shine through the glowing vapour of iron. He employed a method quite as satisfactory however, causing the light from iron vaporised by the electric spark to form a spectrum side by

line for strong line, and faint line for faint line, every line of the iron spectrum appears as a dark line in the spectrum of the Sun!

Kirchhoff in all probability never questioned for a moment the conclusiveness of this relation. He must have felt its significance intuitively. It was probably, therefore, only to satisfy others that he presented a strong argument from the theory of probabilities in favour of the view that the observed relation implied a real association between the two sets of lines. Taking as $\frac{1}{2}$ the chance that a bright line in the iron spectrum would *seem* to have a counterpart in the richly 'lined' solar spectrum if accident were alone in question, he calculates that the chance of the observed relation (leaving altogether out of question the relative strength of the lines) is less than

$$\frac{1}{1{,}000{,}000{,}000{,}000{,}000{,}000.}*$$

'Hence,' remarks Kirchhoff, 'this coincidence must be produced by some cause, and a cause can be assigned which affords a perfect explanation of the phenomenon.

side with the solar spectrum, the solar light being admitted directly to the battery of prisms; that of the iron being admitted after reflection through a small prism near the slit.

* To this I may add that the chance of the observed relation, now that 450 lines of iron are recognised, is less than a fraction whose numerator is unity, and whose denominator consists of no less than 136 figures (the first four being 2907). This chance is not very unequal to that which I have shown to correspond to the probability that one of the less marked peculiarities of stellar arrangement detected by me while constructing my larger star-atlas, is due to mere chance-distribution.

The observed phenomenon may be explained by the supposition that the rays of light which form the solar spectrum have passed through the vapour of iron, and have thus suffered the absorption which the vapour of iron must exert. As this is the only assignable cause of the coincidence, the supposition appears to be a necessary one. These iron vapours might be contained either in the atmosphere of the Sun or in that of the Earth. But it is not easy to understand how our atmosphere can contain such a quantity of iron vapour as would produce the very distinct absorption-lines which we see in the solar spectrum, and this supposition is rendered still less probable by the fact that these lines do not appreciably alter when the Sun approaches the horizon. It does not, on the other hand, seem at all unlikely, owing to the high temperature which we must suppose the Sun's atmosphere to possess, that such vapours should be present in it. Hence the observations of the solar spectrum appear to me to prove the presence of iron vapour in the solar atmosphere with as great a degree of certainty as we can attain in any question of natural science.'

Thus cautiously did Kirchhoff proceed in establishing the great principle on which spectroscopic researches into solar physics were to depend. Nothing can perhaps be required to strengthen the confidence of the reader in the justice of this principle. Yet it may be added that the effect of our own atmosphere on the solar spectrum is now thoroughly recognised and found to correspond with the density of the layers through

which the Sun's rays penetrate at different hours of the day. The reader may further be reminded of what has been already said respecting the evidence afforded by the character of stellar spectra. All those spectra would undoubtedly be similar to each other and to the solar spectrum if our atmosphere contained the source of all the spectral dark lines. It is to be noted also that while the coincidences in the case of iron serve to confirm any evidence we may derive from other coincidences, they are also *confirmed by* such evidence; because as one after another the terrestrial elements are found to have spectral lines corresponding with the solar dark lines, the general fact becomes more and more firmly established that the Sun is constituted of those elements with which we are familiar.

Kirchhoff found that calcium, magnesium, and chromium exist in the solar atmosphere. The presence of nickel, also, and cobalt was indicated by the agreement of the most conspicuous of their lines with solar dark lines. All the lines of these metals, however, could not be recognised, nor were the coincidences in the case of cobalt sufficient to satisfy Kirchhoff. 'I consider myself entitled,' he says, 'to conclude that nickel is visible in the solar atmosphere; but I do not yet express an opinion as to the presence of cobalt. Barium, copper, and zinc,' he proceeds, 'appear to be present in the solar atmosphere, but only in small quantities; the brightest of the lines of these metals correspond to distinct lines in the solar spectrum, but the weaker lines are not noticeable. The remaining metals which

I have examined, viz. gold, silver, mercury, aluminium, cadmium, tin, lead, antimony, arsenic, strontium, and lithium, are, according to my observations, not visible in the solar atmosphere.'

I have thus far quoted Kirchhoff, not because the account of his investigations exhibits the actual state of our knowledge at the present time, but because they are so associated with the discovery of the great principle on which spectroscopic analysis depends as to have an interest wholly distinct from that—great as it is—which they derive from their intrinsic importance. It would occupy much more space than is here at my disposal to exhibit the progress of research by which our knowledge of the solar spectrum has reached its present position. For the full history of the subject I would refer my readers to Dr. Schellen's admirable 'Spectralanalyse,'* and to the excellent treatise on spectroscopic analysis by Professor Roscoe. So far as my purpose in this place is concerned, it is only necessary for me to sum up the results of the search for coincidences between solar dark lines and the bright lines of terrestrial elements—in other words, to exhibit the elements which exist, so far as is yet known, in the substance of the great central luminary of our system. The following table, drawn up by M. Ångström,

* A translation of the second edition of this work by the daughters of Mr. Lassell, F.R.A.S., and edited by Dr. Huggins, the great English master of the subject, published by Messrs. Longmans, contains many important additions. It is a work of extreme value, which no one should be without who wishes thoroughly to master the subject of spectrum analysis.

exhibits the number of lines belonging to the several elements enumerated, which have been found to correspond with dark lines of the solar spectrum:—

Hydrogen	4	Manganese	57
Sodium	9	Chromium	18
Barium	11	Cobalt	19
Calcium	75	Nickel	33
Magnesium	4 + (3?)	Zinc	2
Aluminium	2?	Copper	7
Iron	450	Titanium	200

It will be noticed that the solar spectrum shows no traces of the existence of the nobler metals, gold and silver, nor of the heavy metals, platinum, lead, and mercury. On the other hand, it is significant that the lines of nitrogen and oxygen are absent, though these gases can scarcely be supposed to be actually wanting. We must remember, in forming an opinion as to the *absence* of these elements (as also of such elements as carbon, boron, silicon, and sulphur), that while the presence of certain lines in the solar system may prove abundantly that the terrestrial element which has corresponding bright lines, exists in the Sun's substance, it by no means follows with equal certainty that because all the lines of an element are wanting in the solar spectrum, therefore the Sun does not contain those elements. This will appear when we consider the various circumstances which may cause an element really existing in the Sun's substance to afford no trace of its presence. In the first place, the vapour of that element may be of a density causing it to lie always at a very low level, and therefore perhaps

altogether beneath that level whence proceeds the white light of the Sun—that is, the light which gives the continuous spectrum across which the dark lines lie. Or again, the element may exist in the Sun's substance at such a temperature, or at such a pressure, as to produce—not well defined absorption lines, but —only broad faint bands, which no optical means we possess can render sensible as such. Or, again, the element may be in a condition enabling it to radiate as much light as it absorbs, or else very little more or very little less—so that it either wholly obliterates all signs of its existence, whether in the form of dark lines or bright lines, or else gives lines so little brighter or darker than the surrounding parts of the spectrum that we can detect no trace of their existence. In these, and in yet other ways, elements may really exist (or rather undoubtedly do exist in the Sun) of whose presence we can obtain no trace whatever.

In other chapters of this work the evidence afforded by the spectroscope respecting the condition of various parts or appendages of the Sun, as the spots, faculæ, pores, prominences, corona, and so on, will be considered at length, as also the various theories to which such evidence has given rise. I conclude this chapter by enunciating the general rules of spectral analysis, and by explaining the rationale of certain recent applications of the spectroscope which have deservedly attracted great interest, but are not perhaps very generally understood.

The general principles of spectroscopic analysis are as follow:—

1. An incandescent solid or liquid gives a continuous spectrum.

2. A glowing vapour gives a spectrum of bright lines, each vapour having its own set of bright lines, so that from the appearance of a bright line spectrum we can infer the nature of the vapour or vapours whose light forms the spectrum.

3. An incandescent solid or liquid shining through absorbent vapours gives a rainbow-tinted spectrum crossed by dark lines, these dark lines having the same position as the bright lines belonging to the spectra of the vapours.

4. Light reflected from any opaque body gives the same spectrum as it would have given before reflection.

5. But if the opaque body be surrounded by vapours, the dark lines corresponding to these vapours appear in the spectrum with a distinctness proportioned to the extent to which the light has penetrated these vapours before being reflected.

6. If the reflecting body is itself luminous, the spectrum belonging to it is superadded to the spectrum belonging to the reflected light.

7. Glowing vapours surrounding an incandescent body will cause bright lines or dark lines to appear in the spectrum according as they are at a higher or lower temperature than the body; if they are at the same

temperature, they will emit just so much light as to compensate for that which they absorb, in which case there will remain no trace of their presence.

8. The electric spark presents a bright-line spectrum, compounded of the spectra belonging to those vapours between which and of those through which the discharge takes place. According to the nature of these vapours, and of the discharge itself, the relative intensity of the component parts of the spectrum will vary.

It will be seen, as we proceed, that all these principles bear more or less directly on the application of spectroscopic analysis to the interpretation of solar phenomena.

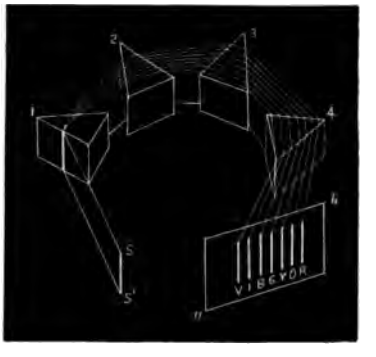
In all branches of spectroscopic research certain difficulties have to be specially dealt with, while certain circumstances avail to help the observer. We are now to consider,—first, the means which are available to the astronomer for the special purpose of advancing our knowledge of solar physics; and, secondly, the peculiar difficulties which he has to overcome.

In the experiment illustrated in fig. 21 we see the action of a single prism on the solar light. But the time has long since passed when the action of a single prism could teach us anything new respecting the Sun. The observer has to work nowadays with a battery of prisms. A portion of light which has been already dispersed by one prism is received on another, and is thus yet more dispersed; a portion of this doubly dispersed light falls again on another prism and emerges yet more dispersed; a portion of this trebly

dispersed light falls on a fourth prism, and so on—the number of prisms actually employed depending on the special branch of inquiry which is being pursued, for some require more dispersive power than is necessary for others.

Space compels me to limit my description specially to the action of the prismatic battery, and I therefore neither describe nor illustrate the means adopted for causing a suitably-shaped beam of solar light to fall on

FIG. 25.



the first face of the first prism. I therefore simply state the fact that, in the study of the solar spectrum, a beam (of parallel rays) whose cross section is as s s′ (fig. 25), the shape of the slit through which light is admitted, falls, as shown in fig. 25, upon the first prism of the battery, or prism 1 in the figure. If a screen were placed to intercept this beam anywhere between s s′ and prism 1, there would be seen upon the screen a bright bar of light shaped like s s′. Prism 1 disperses this beam in the manner already described, and

the beam falls thus dispersed on prism 2. If a screen were placed to intercept the beam anywhere between prism 1 and prism 2, a short solar spectrum would be seen on it, the violet end lying towards the bases of the prisms; and, assuming the battery of prisms to lie on a horizontal plane, the length of the spectrum—that is, its extension measured from red to violet—would be horizontal. Then this beam passes to 3; and if a screen were placed between 2 and 3 a somewhat longer spectrum would be seen. Between 3 and 4 the spectrum would be still longer. And lastly, a screen placed beyond the last prism, as *a b*, would show the solar spectrum much longer and proportionately fainter than in Newton's experiment. And although no such screen is used by spectroscopists, who receive the emergent rays into a telescope with which they examine the spectrum, it will be convenient for our purpose to refer at present to a spectrum supposed to be received on a screen, as in fig. 25.

Now, on *a b* I have shown a violet image of the slit V, an indigo one I, and so on, to a red image at R. But there are an infinite number of images ranged from the extreme violet end to the extreme red end. In places, however, no images are formed. The spaces thus left without light are the dark lines.

It is not difficult, then, to see on what conditions the visibility of the dark lines will necessarily depend. If we could have a slit which was a true mathematical line, every dark space would be present in the screen, even though the dispersive power were small. But, as

a matter of fact, the slit has a definite breadth, however narrow we may make it. Now, suppose that A B C (fig. 26) represents a small part of the solar spectrum as shown on the screen in fig. 25, but that the *true* nature of this part of the solar spectrum * is shown in the narrow band *a b c*, so that in reality sunlight has no rays whose refrangibility corresponds to the band at *b*. Suppose, further, that the aperture of the slit is equal in width to this band *b*. Then the light corresponding to the extreme limits of the light in *a b c*, will form the two images of the slit shown at B; these

Fig. 26.

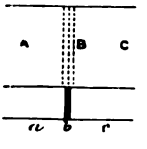
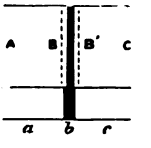


Fig. 27.



images will meet, and no absolutely black line will be seen. There will, however, be a dusky band, darkest down the middle, and twice as broad as the true band *b*. But now consider the effects of an increase of dispersive power. Say we double the length of the spectrum, or rather of the particular part shown in figs. 26 and 27. Then the parts *a b c* of the real solar spectrum will all be doubled in length, and in the observed solar spectrum the light corresponding to the extreme limits

* The true solar spectrum may be regarded as that corresponding to the imaginary case of a slit, which should be a true mathematical line, and of a battery of prisms which should cause no optical faults whatever, insomuch that the image of the slit for rays of any order of refrangibility would also be a mathematical line.

of the band *b* will produce the two images of the slit shown at B and B′, but these will be no wider than before, and will be separated by a really black band, half as wide as *b*. This band will be bordered by a penumbral fringe whose boundaries are indicated by the dotted lines, the whole breadth from dotted line to dotted line being half as great again as that of the band *b*.

The reader will, therefore, at once see the importance of increasing the dispersive power of our battery of prisms; since in this way very fine lines which might otherwise escape detection can be rendered visible. Also, it is obvious that two lines very close together would be shown as one with a certain amount of dispersive power, while with more dispersive power they would be clearly separated. Yet once more, a line in the solar spectrum which *seemed* to coincide, without being really coincident, with the bright line of some metallic spectrum (brought into comparison with the Sun's in the manner referred to above) might, by increase of dispersive power, be removed appreciably from its supposed counterpart.

So far, then, as the direct examination of the solar spectrum is concerned, we must aim specially at the means of increasing dispersion. It will be seen, also, further on, that in two highly important applications of the analysis—viz. to the examination of the prominences, and to the recognition of motions of approach or recess due to solar cyclones—great dispersion is the chief point to be secured by the spectroscopist.

But fig. 24 shows that certain difficulties have to

be encountered in securing great dispersion. Only the part of the spectrum near G has been formed (in the case illustrated in that figure) by rays which have gone symmetrically through the battery of prisms. The rays forming the part near V have gone through the middle of prism 1, and come out near the base of prism 4; while those forming the part R passing also through the middle of prism 1, come out near the vertex of prism 4. Now, optical considerations render it extremely important as far as the clear definition of the lines is concerned, that each part of the spectrum should be formed by rays which have gone symmetrically round such a battery as is shown in fig. 24.* Furthermore, when the dispersion is very great only a portion of the spectrum will be formed at all (some at either extremity falling outside the last prism—beyond apex and base) when the prisms occupy a fixed position. Then, again,

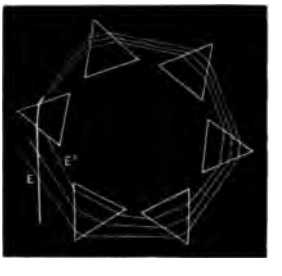
* The point to be secured is that the rays forming any part of the spectrum under examination should pass into and out of each prism at equal angles (as the light forming the part G of the spectrum in fig. 25 does). It may be shown that in this case those special rays pass with least possible deviation through each prism, so that the condition is generally called that of *minimum deviation*. But minimum deviation *per se* has no advantages, and, as a matter of fact, the real condition secured by this arrangement is that the primary and secondary foci of emergent pencils are as nearly as possible coincident; so that, though the image of the slit formed by those special rays is not formed by absolute points of light, it is formed by circles (technically called 'circles of least confusion') having the smallest possible diameter. In a paper in the *Monthly Notices of the Royal Astronomical Society*, vol. xxx., I have shown that the circle of least confusion has in this case a definite, though exceedingly minute diameter, even in the case of a single prism. The mathematical expression for the radius of this circle is given in that paper, but is somewhat too complex to be repeated with advantage in these pages.

when the light has been bent round a nearly complete circle of prisms, as in fig. 28, the emergent light E E′ will be intercepted by the first prism of the battery; and this circumstance limits the dispersion which can be given in this manner.

Let us consider how these difficulties have been or may be encountered.

We owe to Mr. Browning * the invention of a most ingenious plan by which, whatever part of the solar

Fig. 28.



spectrum is studied, the battery of prisms will be properly adjusted. In his automatic spectroscope he attaches to each prism a slotted bar, as shown in fig. 29. All the slots pass over a central pivot, and the prisms are attached at their angles as shown. Hence they must necessarily be at all times symmetrically placed; so that if 1 be fixed and motion be communicated to 4, then 2 and 3 will move, each in its proper degree, and all four will preserve their proper relative

* Rutherfurd, the eminent American astronomer, had earlier devised and used an instrument on a somewhat similar plan; but Mr. Browning's new contrivance was independently devised, and in one important respect was an improvement on Rutherfurd's.

positions. In Browning's spectroscope there are six such prisms, and the light emerging from the sixth passes just clear of 1.

I have suggested a modification, by which only a

FIG. 29.

corner of the first prism is fixed, as at fig. 30, and this prism is automatically adjusted like the rest,—a fixed slot *s s'* square to the path of the incident light guiding the motion of this prism according to the required

FIG. 30.

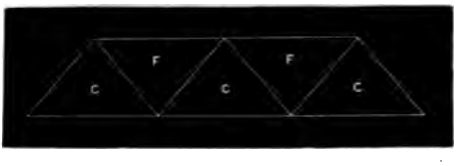
law. I have suggested, also, that the viewing telescope should be guided on the same principle of automatic motion.

So far as clearness of definition and the satisfactory study of the whole length of the spectrum are concerned, this arrangement leaves nothing to be desired. But for a yet greater increase of the spectrum's length more

is needed. We have reached the limits of dispersion which one circular battery can give; but it would be desirable to obtain a yet greater dispersion.

One way in which this can be done is by the use of what are called direct-vision prisms. In these, two flint-glass prisms (F F) are combined, as shown in fig. 31, with three crown-glass prisms. The prisms C cause deviation and dispersion in one direction; the prisms F cause equal deviation and more dispersion in

FIG. 31.



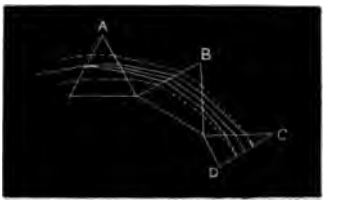
the contrary direction. Hence results a balance of dispersion without deviation; and if we add such a prism to any battery of prisms, we get all the advantage of the dispersion without increasing the deviation which had been our difficulty.

Yet there is one important disadvantage in direct-vision prisms, more especially when they are employed in researches requiring very neat and exact definition: it is, of course, wholly impossible to employ any method for securing minimum deviation.

A plan by which the dispersion in a battery of prisms may be doubled, has next to be considered. Supposing A and B (fig. 32) to be two of the ordinary triangular prisms and C a right-angled prism half the size of the others, then the rays which fall on C D are

reflected back again through the battery. I need not here describe the contrivance by which the emergent pencil can be viewed in such a case by a telescope in which there is a totally reflecting prism sending the light out at right angles to the axis of the tube. According to another arrangement, in place of such a prism as C, one is employed which by two total reflec-

FIG. 32.

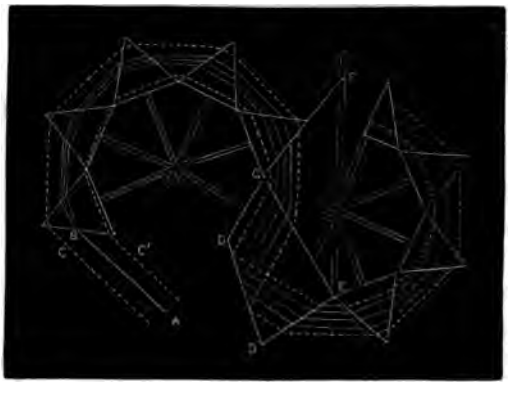


tions raises the rays to a higher level, so that (the prisms of the battery being twice as high as usual) the light returns by a separate course.

I have devised a plan by which a much greater dispersion than has ever yet been gained may be combined with a perfectly true automatic adjustment for all parts of the spectrum. In this plan Mr. Browning's automatic method is extended to a second battery, while the plan for returning the rays, illustrated in fig. 33, is employed in such sort as to double the dispersive power of the double battery. Fig. 33 shows the arrangement of the two batteries. A B is the light incident on the first battery, and the course of the light can be traced by the triple set of lines through the double set of prisms, the dotted return lines show-

ing the course of the return rays to emergence at C C′. It will be seen that the large intermediate prism D E may be regarded as belonging to both batteries. There is no loss of light in passing from one battery to the other, since the reflection at D D′ is total. The close double lines show the direction of the slotted bars, E F being a long slotted bar kept square to the rays leaving the face D′ E by means of the equality of G F and G E. (It pivots round E and slots at F.)* The figure shows

FIG. 33.



the arrangement of a battery having an effective dispersing power equal to that of nineteen equilateral prisms of heavy flint-glass.†

* An instrument on this plan has been constructed by Mr. Browning, to whom I submitted the proposal. The mechanical arrangements had, as I expected, to be slightly modified, the bar E F being replaced by a radial bar carrying the last prism of the second battery.

† The double battery is represented in about the position corresponding to minimum deviation for the line G in the indigo; and the automatic motion can be carried on till the visible extremity of the spectrum to-

Now there are optical considerations relating to the method in which the slit admits light, which need not here be specially dealt with. But it is important to notice that a spectroscope may do one of two things. Either it may form the spectrum corresponding to light reaching the slit from a considerable range (as for instance the whole space occupied by the sun, and perhaps the illuminated sky for a width of two or three degrees) *or else* it may give the spectrum corresponding only to a narrow line of light. In one case the spectrum is produced by the spreading out as it were of multiplied images of the illuminated slit, the illumination of the slit being derived from a wide area of light. In the other the spectrum is formed of multiplied images of the line of light under examination. A spectroscope serving the former purpose may be called (as Professor Young, of America, suggests) an *integrating spectroscope*; one of the latter kind an *analysing spectroscope.*

What follows relates to the work of the analysing spectroscope.

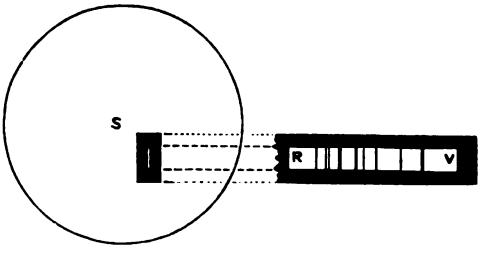
It must be remembered that what the analysing spectroscope really does is to give a range of pictures of whatever luminous object or part of an object would be visible through the slit if the spectroscope were removed.

Now, supposing such a portion of the solar photo-

wards the violet is reached. Of course, in examining the red rays, the batteries open out, and there is no limit (except the length of the slots) to motion in that direction, so that the red extremity of the spectrum can be overpassed if need be.

sphere is observed as is shown in the space *s s'*, fig. 34 (s being the solar disc), the images of this portion will give us the spectrum R V, showing the dark lines due to the absence of certain images, as explained above;

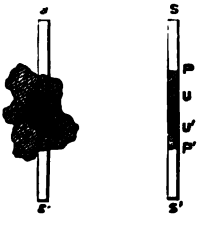
Fig. 34.



and *no other portion of the disc produces any effect whatever.* It is very essential to remember this. We are in fact analysing under such circumstances the part *s s'* of the disc, and no other part.

If a spot or a facula be crossed by *s s'*, then the spectrum we get is no longer that of a uniformly, or almost uniformly, bright part of the solar disc. If *s s'*

Fig. 35.



(fig. 35) represent an enlarged view of the spot and the space included by the slit, then this last, seen sepa-

rately, will be as S S′; and the spectrum will consist of a number of images of S S′ ranged side by side, so as to form a strip, as R V in fig. 34. Hence at the top and bottom of this compound spectrum there will be two narrow solar spectra corresponding to the parts S P and S′ P′; next to these will be two narrow spectra of the penumbral parts P U and P′ U′; and about the middle there will be a narrow spectrum corresponding to the umbral part U U′, all these spectra forming one compound spectrum, whose red end is towards the left (assuming the dispersion to be as in the case illustrated in fig. 34) and its violet end towards the right. The nature of the penumbral and umbral spectra will be stated further on. It is by comparing these spectra with the adjacent solar spectra that the spectroscopist is enabled to form an opinion as to the nature of the spots, and to make inferences as to the general physical constitution of the Sun.

Similar remarks apply to the case where a portion of facula, or pores, or mottlings, or of any other features of the solar disc, falls within the space *s s′*. All such peculiarities tend to produce peculiarities in the resulting compound spectrum, since the image of the portion *s s′* is repeated along the whole length of the spectrum R V after the fashion already described.

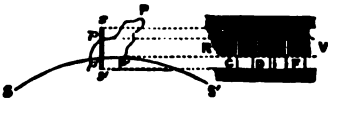
But, having considered these comparatively simple cases, let us deal with the subject which has of late attracted so much attention—the visibility of the spectrum of the prominences when the Sun is not eclipsed. Further on the exact nature of the prominence-spec-

trum will be considered; but in this place I note only respecting it that it consists of bright lines.

Now, suppose that P P′ is a prominence, S S′ the edge of the Sun, and *s s′* the space included by the slit. Then *p′ s′*, as in the former cases, produces a solar spectrum (which, however, commonly presents certain peculiarities when belonging to the edge of the Sun's disc); the part *p p′* includes a portion of the prominence, and gives a prominence-spectrum which we may suppose to be represented by the bright lines at C and F and near D. But it will also give a solar spectrum, for the light of our own illuminated air comes from the space included within the slit *s s′*; and as our air is illuminated by solar light, it produces (according to rule 4, page 127) a solar spectrum. Also the part *s p′* will give a solar spectrum due to the illuminated air. Now, the prominence P P′ is absolutely obliterated from view by the illuminated air, which extends all round (and over, be it remembered) the place of the Sun. Since then, if we looked at the space *s s′* alone, with whatever telescopic power, and with whatever contrivances for reducing the glare of light, the portion *p p′* of the prominence P P′ would be wholly invisible to us, *why*, it may be asked, should the lines C, F, and the one near D—which are in truth but coloured images of the part *p p′*—be visible, although the spectrum of the illuminated air falling within *s p′* is spread over these lines precisely as the illuminated air is itself spread over the prominence? The answer is easy. The light of the illuminated air

within the small space $s\ p'$ is spread over the large space shaded with cross lines in fig. 36, and is reduced in intrinsic brightness in corresponding proportion. On the other hand, the light of the prominence-matter

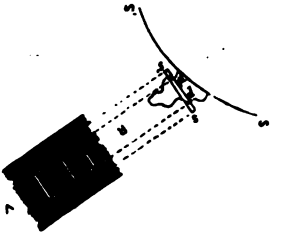
Fig. 36.



within $p\ p'$ is spread only over the three lines shown in the figure (and a few fainter ones), and is therefore proportionately but very little reduced. Hence, if we only have enough dispersive power, we can make sure of rendering the prominence-lines visible, for we get the same luminosity for them whatever the length of the spectrum, the only effect of an increase of length being to throw the bright lines farther apart; whereas the atmospheric spectrum which forms the background will obviously be so much the fainter as we spread its light over a longer range.

By this plan we get a certain number of images of a

Fig. 37.



portion of a prominence—a mere strip, so to speak; and

we can get any number of such portions, and in any direction as compared with the Sun's limb. For example, if S S′ (fig. 37) be the Sun's limb, P P′ a prominence, we can get from such a strip as *s s′* the spectrum R V. And obviously since the length of the bright lines tells us the length of the part *p p′* in figs. 36 and 37, we can, by combining a number of such parallel strips as S S′, learn what is the true shape of the prominence P P′.

But the plan can be applied to show the whole of a prominence. For let us suppose that in place of a narrow strip as *s s′*, in figs. 36 and 37, we have a space such as is shown in fig. 38, through which, but for the intense brightness of the illuminated air, the prominence P P′ would be visible. Then the part S S′ of the Sun will produce a solar spectrum—altogether impure, of course, on account of the great width of S S′, and brighter than the solar spectrum produced by *s′ p′* in the case illustrated by fig. 36 in precisely the proportion that S S′ in fig. 38 is greater than *s′ p′* in fig. 36. All the remainder of the space, including the prominence P P′, will give an impure solar spectrum due to

Fig. 38.

the illuminated air, and very much brighter than in the cases illustrated in figs. 36 and 37, because so much more of this light is admitted through the open slit.

Three coloured images will be formed of the prominence (other fainter ones need not be considered), one red at C, one orange near D, the other greenish-blue near F. These images will be as bright (neglecting variations in the intrinsic brilliancy of the prominence) as the corresponding lines in the cases illustrated by figs. 36 and 37; but they will of course not be so well seen, since the background, as I have said, will be very much brighter than in those cases. They can be made as conspicuous only by an increase of dispersive power; hence the importance of constructing prismatic batteries of great dispersive power.

In connection with this portion of my subject, it is necessary to remark that the bright lines seen in the prominence-spectrum are not uniformly wide throughout, but commonly are wider close to the Sun's limb. This circumstance will be referred to more at length further on; but it is proper to state in this place, that this increase of width is held to indicate an increase of pressure, because the researches of Plucker, Hittorf, Huggins, and Frankland have shown that the spectral lines of hydrogen grow wider as the pressure at which the gas exists is increased.

And now, lastly, it remains that I should explain what has been justly regarded as the most wonderful of all applications of the power of spectroscopic analysis —the measurement of the velocity of recess or approach of stars, or other self-luminous objects moving with very great rapidity.

Reverting to fig. 21, the reader will see that the

violet rays are most affected by their passage through the prisms, the red rays least. Now, it has been demonstrated by the careful mathematical analysis* of the motions of light-waves that this difference of refrangibility is due to the different velocities with which the longer light-waves forming red light, and the shorter light waves forming violet light, travel (respectively) through material media.† The shorter waves travel more slowly than the long ones, and the difference is the greater according to the density (or approach to opacity) of the medium. So that, in fine, the part of the spectrum formed by light of any order depends on the wave-length of that light; and if under any circumstances the wave-length could be altered, then the light of that order would no longer occupy the same portion of the spectrum, but would pass nearer to the violet end if the waves were shortened, and nearer to the red end if they were lengthened.

Now, so far as we know, it never happens that light-waves of a certain length are really modified. Precisely as waves of a certain breadth propagated along a canal are not found to change their breadth as they proceed, or, again, precisely as a sound of a certain tone

* I refer here to the investigations of Cauchy, and Baden Powell, and others (see specially Cauchy's *Mémoire sur la Dispersion*), not, of course, to the proof that when the differences of velocity are admitted, differences of refrangibility are accounted for. The latter may be regarded in fact as self-evident.

† In the ether of space they travel of course with appreciably equal velocities; otherwise the satellites of Jupiter, after emerging from eclipse, would show the same changes of colour that we see in a metal heated from a red to a white heat.

does not change in tone as it travels onwards, so light-waves of a certain length or order do not as they travel through ether, or through material media, become changed into light-waves of some other order.*

But there is a circumstance which may cause the light-waves to *appear* to change in length. Supposing the source of light is approaching or receding at a very rapid rate—at a rate which bears an appreciable proportion to that of light—then the length of the light-waves must needs appear modified—shortened when the source of light is approaching, lengthened when it is receding. The same will also hold if the observer be carried very rapidly towards or from the source of light. To see that this is so, it is only necessary to consider that more light-waves must necessarily reach the observer in a given time when the source of light is approaching, than when it is at rest (with respect to him), and fewer when it is receding. They must then in one case succeed each other more rapidly, and so seem to be separated by shorter intervals, while in the other

* I have sometimes been inclined to suspect, however, that under certain circumstances of excessive agitation within the substance of the source of light, the wave-length might be altered, precisely as waves travelling along a canal might be modified in length by the action of the cause which gave them birth. When we know that the C line of the prominences has been observed to be tranquil, while the F line has been broken, the idea is certainly suggested that those molecular motions within the substance of the hydrogen of the prominences, which produce that part of the light corresponding to the F line, may by some violent action be so far modified that the observed disturbance of the wave-length corresponding to that particular line may be brought about. It seems difficult to understand how, under any other circumstances, one line of the hydrogen should be undistorted, while the other is, to use Professor Young's description, 'absolutely shattered.'

they must succeed each other more slowly, and so seem to be separated by longer intervals.*

* The principle on which this explanation depends admits of several illustrations. The following is quoted from a paper (the first matter I ever wrote for publication) which appeared in the *Cornhill* for December, 1863:—'Let the reader imagine himself on the bank of a canal, observing a series of waves uniformly propagated along the stream. . . A very simple method will suffice to determine the breadth of the waves with any required degree of accuracy. Let the observer, fixing his eye on a certain wave, walk any measured distance (say 100 yards) at the same rate as the wave is moving. Suppose he accomplishes this distance in 65 seconds. He knows then that the velocity of transmission of the waves is 100 yards in 65 seconds. Let him now, standing still for 65 seconds, count the number of crests that pass him in that time. Suppose 360 pass him. Then, from his first observation, he knows that the first which passed him has travelled 100 yards from him. Within that distance all the 360 waves are uniformly distributed. Thus the breadth of each is 1-360th part of 100 yards, or ten inches. This result is perfectly reliable if, during his second observation, his position on the bank has been unchanged. But let us imagine that he has made this observation from a truck—on rails by the canal's edge—and that, unnoticed by him, the truck has glided uniformly along the rails. First, suppose that this motion has taken place in a direction contrary to that of the waves, and that while he is counting the passing crests the truck glides a distance of 20 yards. It is evident that when the last wave passes him, the first is 120 yards, instead of 100, from him. Thus the 360 waves are distributed over 120 yards, and the true breadth of each is 1-360th part of 120 yards, or twelve inches. If, on the other hand, the truck had moved over 20 yards in the same direction as the waves, it is equally obvious that the 360 waves would be distributed over only 80 yards, and the breadth of each would be only eight inches. Similarly, at whatever rate the truck moves, it is evident that the observer can no longer depend on the result of his observations. If it moves in a direction opposite to that in which the waves travel, they appear narrower; if it travel with them they appear broader than they really are. Indeed, it is not difficult to conceive the truck to move in the same direction and at the same rate as the waves travel, in which case (if we could suppose the observer to remain unconscious of that motion) all undulation would appear to him to have ceased, and the water to have a waved but unmoving surface.'

This account illustrates in a direct (and I think distinct) manner the effect of the approach or recess of a source of light. We see that the

Now, Doppler, who first called attention to this circumstance, supposed that an alteration of colour would result; and this would, in fact, be the case (though the alteration would, under any conceivable conditions, be wholly inappreciable) if the source of light emitted rays of a certain order only. But in the case of such a source of light as a star—or the Sun—no change of colour could be produced, because though—to take the case of approach—the red light would be shifted towards the orange, while a portion of the violet would disappear, yet heat rays from beyond the red end would become visible as red rays through being shortened, and so the spectrum would be complete as before. A similar result would follow in the case of recession.

effect depends on the ratio which the velocity of approach or recess bears to the velocity with which the waves travel. It will be seen at once that equal velocities of approach and recess produce equal but not *corresponding* effects. For, by the supposed velocity of 20 yards in 65 seconds, the length of the waves is increased or diminished by two inches, but the increase is only in the ratio of 6 to 5, while the decrease is in the greater ratio of 5 to 4. Corresponding to this, we have the fact that the approach of a star at a given rate produces a greater relative effect on wave-lengths of any order than the recess of the star at the same rate would produce.

Of course, in the interesting case of stellar approach or recess, the problem is somewhat complicated by the Earth's own motion, which does not take place in the direction of the star, nor necessarily in the same plane. The successful measurement of the velocity with which Sirius is receding from us is a problem of such interest that I may be permitted to note a very small contribution of mine to the work, in the determination of the formula for eliminating the effects of the Earth's motion (v) viz.—Earth's motion towards star $= v \cos \lambda \sin (l - l')$ where l and l' are the respective longitudes of the star and the Earth, and λ is the star's latitude. The contribution has no particular value, but, such as it is, it chanced that I made it.

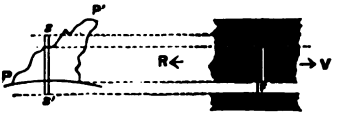
But the presence of dark lines in a spectrum gives the observer a far more effective means than mere change of colour would supply of determining the approach or recession of a source of light. If some recognised dark line in the spectrum of a star—say, for instance, the F line—is found not to agree exactly in position with the corresponding line in the spectrum of a fixed source of light—as a hydrogen flame, for instance—then the difference of position must be ascribed to a motion of recess or approach on the part of the star, and the rate of such motion may be determined by noticing the amount by which the line is displaced.

Now, this method admits of being applied under exceptionally favourable conditions to the examination of solar cyclonic motions—if only these motions are sufficiently rapid to fall within the province of this special mode of research. For we have in the lines appertaining to parts of the Sun which are relatively at rest the means of determining very surely, and measuring somewhat exactly, the displacement due to such motions as we are considering.

Let us take as an instance the case represented in fig. 39. Here *s s'*, as before, represents the portion of the prominence P P' which is under examination. Only a small part of the spectrum is shown, that, namely, near the F line; and the bright line of the prominence which under normal conditions coincides with the dark line F of the solar spectrum is seen to be displaced towards the violet. It thus appears that, owing to a motion of approach affecting the portion of

the prominence included within the narrow space *s s'*, the light-waves producing the F line seem shortened. The general fact of a motion of approach is thus ascertained. But the rate of approach can also be measured; for we know the length of the light-waves corresponding to the line F, Van der Willingen and Ångström having independently determined the wave-length corresponding to the principal lines in the solar spectrum. So that if we measured in any way the distance of the bright F line of the prominence from the dark F line of the solar spectrum, we should be able

FIG. 39.



to calculate the amount of the apparent change. A better plan, however, is available. For the bright solar spectrum (fig. 39), as also the atmospheric spectrum above, is crossed by other dark lines besides the F line, and these enable us to see at once how far the bright line has shifted. Suppose *l*, for example, to be another dark line of the solar spectrum, and that the bright prominence-line has moved half-way from its proper place towards *l*, then we know that its wave-length is changed to a value midway between the wave-length corresponding to the lines F and *l*.* The

* This is true for such small displacements as are here considered. For greater differences of refrangibility, no such simple proportions exist, partly because the actual change of wave-length (for given differ-

change of value thus indicated gives us at once the rate of the motion of approach which affects the portion of the prominence-matter corresponding to the space *s s'* *—because, though the wave-length corresponding to the line *l* will not be indicated in the tables of Ångström or of Van der Willingen (which only include the principal lines), yet it is readily determinable, and indeed may be regarded as a known quantity. And, in a similar way, if the line is shifted towards the red end, the velocity of recession of the prominence-matter can readily be determined.

But as a general rule the whole line would not be shifted bodily as in fig. 39; since indeed this would imply that the *whole* of that portion of the prominence which is seen within the space *s s'* was travelling bodily towards the observer; whereas obviously such a motion could very seldom be expected to occur. Ordinarily, then, we may expect to find a configuration of the bright line indicating varieties of motion. The same holds also in the case of portions of the solar photosphere, or spots, lying near the edge of the Sun's

ences of refrangibility) diminishes towards the violet end, and partly on account of the irrationality of dispersion for all known media, when the spectra they give is compared with what Ångström has called the *normal* solar spectrum.

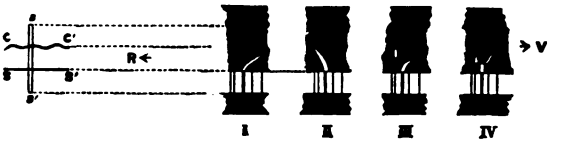
* The general rules on which the calculation proceeds are sufficiently simple. Suppose the wave-length corresponding to the line *l* to be 485·89 millionths of a millimeter, that corresponding to the line F being 486·39 such millionths. Then, since the prominence F-line appears half-way between F and *l*, the wave-length has been reduced to 486·19 millionths of a millimeter, or diminished by 0·2 such millionths. Hence the velocity of approach of the prominence matter is $\frac{20}{486\cdot39}$ths of the velocity of light, or some 80 miles per second.

disc (so that ordinary cyclonic motions within them may be capable of being recognised by the method we are considering), or in the case of more central portions of the Sun's disc where ascending and descending motions are taking place, resulting in motions of recess or approach with reference to the terrestrial observer. In all such cases we may expect to find peculiarities in the affected lines, corresponding to varieties in the motion or rates of motion of the parts examined.

I give a few examples, illustrating the way in which such peculiarities are to be interpreted. I consider, for convenience, motions taking place in that coloured envelope (whence the solar prominences seem to spring) which has been called the sierra or chromatosphere :—

Suppose *s s'* to represent the portion of the Sun and chromatosphere under examination, S S' being the Sun's

FIG. 40.



limb, C C' the (invisible) outline of the chromatosphere Now, if the F line appeared as in I., we should conclude that the hydrogen in the part of the sierra under examination was quiescent near the Sun's surface, as far as motions of approach or recess are concerned (though it might be moving very rapidly in a direction square to the line of sight), but that at some distance

from the Sun's surface it was moving very rapidly towards the eye, the rate of motion increasing with the vertical height above the Sun. If, on the other hand, the spectrum appeared as at II. (fig. 40), we should come to a similar conclusion, substituting only a motion of recession for one of approach. If the spectrum appeared as at III. we should conclude that to a certain level above the Sun's limb there was a gradually increasing motion of approach, but that at and above that level there was a motion of recession tolerably uniform in rate to a considerable height. The case would resemble those instances in our own atmosphere where an upper air current blows in a different direction than the air nearer the sea-level. If, lastly, the spectrum appeared as at IV., we should conclude that to a considerable height above the Sun's surface there was no motion of recess or approach; but that in higher regions of the chromatosphere there were masses (within the long range of chromatospheric matter really included in the direction of the visual line) moving both from and towards the eye at an enormously rapid rate. The greater or less width of different parts of the bright line would indicate the greater or less pressure at which the hydrogen existed at the corresponding levels during the time of observation. Hence, a bulb on any part of the bright line would indicate a corresponding layer of relatively compressed hydrogen, while a marked narrowing would indicate a layer of hydrogen existing for the time at relatively low pressure.

Similar considerations apply to the spectroscopic

analysis of solar spots, or of faculous regions of the Sun's surface, or generally of any regions where disturbances may produce solar atmospheric currents of approach or recess. Combining the observed shifting of portions of a spectral line with its observed thickness, and also with its relative brightness or darkness (as indicative of greater or less temperature), we have a means of studying those conditions of motion, pressure, and temperature, respecting which the telescope alone can give us no information whatever.

It is wonderful, indeed, to consider that that analysis of the dark lines of the solar spectrum which seemed half a century ago so unmeaning, those speculations of Doppler which but a quarter of a century ago were rejected by many as wholly fanciful, and those inquiries as to the almost evanescent wave-lengths of light which from the days of Newton downwards had been ridiculed as a complete waste of time and thought, should have resulted, under the labours of Bunsen and Kirchhoff, of Plucker and Huggins, and, finally, of Ångström and Van der Willingen, in a means of dealing with problems so recondite and seemingly so hopeless. By an observation not occupying many seconds any clear-sighted observer, armed by our opticians with adequate spectroscopic power, can measure the swiftness of the solar windstorm, can gauge the pressure of the solar atmosphere, and can estimate the relative temperature of spot and faculæ, of photosphere and chromatosphere, and, lastly, of the higher regions to which eruptions cast those masses of glowing vapour which form the solar prominences.

CHAPTER IV.

STUDY OF THE SUN'S SURFACE.

WE may regard the discovery of the spots on the Sun as the commencement of that long series of telescopic researches to which we owe our present knowledge of the solar orb. It is highly probable, indeed, that spots on the Sun had been seen, and even watched for long intervals, when as yet astronomers were not aided by the powers of the telescope. But there is no reason to believe that the nature of the spots so seen, or even the fact that they are true solar phenomena, had ever been suspected by astronomers.* Whatever opinion we

* Kepler supposed that in lines 441 and 454 of Virgil's first *Georgic*, the solar spots were referred to. For 'if any one,' he reasons, 'should refuse to see anything else than an allusion to our clouds in the words

'Ille ubi nascentem maculis variaverit ortum,'

I shall oppose to the interpretation this other verse:

'Sin maculæ incipient rutilo immiscerier igni.'

But the latter verse is quite as applicable to clouds as the former. As regards the occasional recognition of spots by the ancients, however, there seems less room for doubt. We learn from Father Mailla that the Chinese recorded the appearance of spots on the Sun in the year 321 A.D., and Acosta tells us that the natives of Peru told the Spanish invaders that the Sun's face had in former times been marked with spots. In the year 807, a large spot was seen on the Sun for eight

may form of ancient records of solar obscurations, we must turn to the telescopic discovery of the spots for the real commencement of astronomical researches into the Sun's physical condition.

I do not propose to enter here into the discussion which has been raised respecting the astronomer to whom the credit of having first seen the solar spots is to be assigned. This discussion has been pursued by grave authorities with an earnestness which would really seem to imply that they have regarded the matter as of serious importance. Let us simply recognise the fact that the credit of the discovery is not worth contending about,* and proceed to consider what

successive days, and was supposed by those who were little familiar with the laws of planetary motion to be the planet Mercury. Arago is of opinion, also, that the transits of Mercury said to have been witnessed by Averrhoes, Scaliger, and Kepler himself (May 28, 1607) were only observations of Sun spots. It is worthy of notice that the ancients could very well have observed Sun spots, and have even traced the progress of these spots across the solar disc, had they employed the method which Gassenius adopted in observing the transit of Mercury in 1631. He admitted the Sun's rays into a darkened chamber through a small aperture in a shutter, and thus obtained an inverted image of the Sun, on which, when the transit had begun, he could perceive the disc of Mercury. Although no spots are ever seen which, even in the nucleus, are so dark as Mercury, yet many (or rather all the noteworthy spots), present a much more conspicuous appearance than Mercury in transit. Fabricius, indeed, as we shall presently see, did actually apply this method.

* It has been justly remarked by an eminent astronomer of our own time, that the discovery of the spots was a necessary sequel of the invention of the telescope; and whether Galileo, or Fabricius, or Scheiner, or Harriot, first set eyes on these objects, is a matter which can in no way increase the reputation of any one of these astronomers. To discuss the question of priority seems therefore to be simply a waste of time.

the first observers actually saw—a matter of more moment.

From a work by Fabricius,* we learn that in the commencement of the year 1611, while observing the Sun just after sunrise with a telescope of inconsiderable power, he noticed a black spot upon its disc which he supposed to be a terrestrial cloud. He found, however, that the object, whatever it was, belonged to the Sun. As the Sun rose he had to discontinue his observations, for he possessed no means of mitigating the brilliancy of the Sun's light. 'My father and I,' he says, 'passed the rest of the day and the whole night in great impatience, trying to think what this spot might be. "If it is in the Sun," I said, "we shall no doubt see it again; if it is not, its motion will have carried it away from the Sun's disc, and so we shall be unable to see it." On the next morning, however, to my delight, I saw the spot again. But it was not in the same place—a peculiarity which increased our perplexity. We determined to obtain an image of the Sun on a sheet of paper by permitting his rays to pass through a small hole in a darkened chamber, and in this way we saw the spot quite clearly in the form of an elongated cloud. For three days we were prevented by bad weather from continuing our observations; but at the end of that period we again saw the spot, which had crossed obliquely towards the western side of the Sun's disc. Another smaller one

* Entitled *De Maculis in Sole observatis*, &c., and published at Wittemberg in 1611.

had made its appearance near the eastern edge, and in a few days this second spot reached the middle of the disc. Lastly, a third spot appeared. The three spots vanished in the order of their appearance. I was hopeful that they would be seen again, but yet perplexed by doubts and fears; however, ten days afterwards the first re-appeared on the eastern side of the disc. I knew then that it had revolved completely round (the Sun), and since then I have convinced myself that this is really the case.' Fabricius studied the import of his observations, and came to the conclusion that the spots are probably upon the body of the Sun itself. 'We invite the students of science,' he says, 'to profit by our description; they will doubtless conclude that the Sun has a motion of rotation, as Jordanus Bruno has asserted, and, more lately, Kepler. Indeed, I do not know what we could make of these spots on any other supposition.'

Galileo, at Florence, and Father Scheiner, a German Jesuit, besides independently discovering the spots, investigated the laws which regulate the motion of these objects. Scheiner was at first disposed to regard the spots as due to the existence of planets travelling round the Sun close to its surface, and indeed for a while these imagined planets were admitted as true members of the solar family, under the title of the Borbonian stars.* But Galileo having pointed out

* They are referred to under this name in Burton's *Anatomy of Melancholy* (a very short time after their discovery), in that singularly interesting chapter which he entitles, quaintly enough, a *Digression of*

that the motions of the spots do not correspond to this hypothesis, but point clearly to the conclusion that the Sun rotates on its axis in about a month, Scheiner re-examined his hypothesis, and presently admitted that Galileo was in the right. He then made a long and elaborate series of observations in order to deter-

Ayre. The following passage from this chapter is sufficiently *apropos*, and will, I doubt not, interest the curious reader :—

'In the meantime, the world is tossed in a blanket amongst them; they hoyse the earth up and down like a ball, make it stand and goe at their pleasures. One saith the sun stands; another, he moves; a third comes in, taking them all at rebound; and, lest there should any paradox be wanting, he findes certain spots and cloudes in the sun, by the help of glasses, which multiply (saith Keplerus) a thing seen a thousand times bigger *in plano*, and make it come 32 times neerer to the eye of the beholder; but see the demonstration of this glass in Tarde, by means of which, the sun must turn round upon his own center, or they about the sun. Fabritius puts onely three, and those in the sun: Apelles, 15, and those without the sun, floating like the Cyanean isles in the Euxine sea. Tarde the Frenchman hath observed 33, and those neither spots nor clouds, as Galileus (*Epist. ad Velserum*) supposeth, but planets concentrick with the sun, and not far from him, with regular motions. Christopher Schemer [Scheiner], a German Suisser Jesuit, Ursica Rosa [Qy., in his *Rosa Ursina*], divides them *in maculas et faculas*, and will have them to be fixed *in solis superficie*, and to absolve their periodicall and regular motion in 27 or 28 dayes; holding withall the rotation of the sun upon his center; and are all so confident, that they have made skemes and tables of their motions. The Hollander, in his *dissertatiuncula cum Apelle*, censures all; and thus they disagree amongst themselves, old and new, irreconcileable in their opinions; thus Aristarchus, thus Hipparchus, thus Ptolomæus, thus Albatoginus, thus Alfraganus, thus Tycho, thus Ramerus, thus Rœslinus, thus Fracastorius, thus Copernicus and his adherents, thus Clavius and Maginus, &c., with their followers, vary and determine of these celestiall orbs and bodies; and so, whilest these men contend about the sun and moon, like the philosophers in Lucian, it is to be feared the sun and moon will hide themselves, and be as much offended as shee was with those, and send another message to Jupiter, by some new fangled Icaromenippus, to make an end of all those curious controversies, and scatter them abroad.'

mine the true period of rotation and the actual position of the solar axis of rotation. In his 'Rosa Ursina' (a most monstrous volume)* he published the results of his labours. He assigned to the Sun a rotation period of between twenty-six and twenty-seven days. He also stated that the plane of the Sun's equator is inclined between 6° and 8° to the plane of the ecliptic,—a very creditable result, considering his means of observation, since the best modern measures assign $7\frac{1}{2}°$ as the value of this angle.

Scheiner, Galileo, and Hevelius would seem to have independently recognised the fact that the solar spots are not of uniform brightness, but commonly surrounded by a fringe less dark than the central part. The last-named astronomer also recognised the existence of certain bright streaks in the neighbourhood of the spots. He called these the faculæ.

* Yet not meriting the disparaging comments of Delambre, who says, 'There are few books so diffuse and so void of facts. It contains 784 pages; there is not matter in it for fifty.' The prolixity, however belongs to the age in which Scheiner lived, and is by no means peculiar to him. By a similar mode of judging, we should be entitled to hold in derision the works of Kepler. Lalande thus writes respecting Scheiner: 'Quoi qu'il en puisse être de celui à qui le hasard a fait voir les taches pour la première fois, il est sûr que personne ne les observa aussi bien et n'en donna la théorie d'une manière aussi complète que Scheiner. Son ouvrage a 774 pages sur cette matière, et cela suffit pour faire voir avec quelle assiduité il s'en occupa, et combien il donna d'étendue à ses recherches. Hévélius le cite avec le plus grand éloge: "Incomparabilis et omnigenæ eruditionis .... ut in hac materia omnibus palmam quasi præripuisse dici posset."' So much it has seemed fitting to say respecting a most laborious and ingenious investigator, whose valuable researches into solar physics have not received the credit which they deserve.

A long series of observations of Sun-spots now began, and many hypotheses of more or less ingenuity were put forward to account for the phenomena which they present. For some time, indeed, the possibility of their existence was earnestly denied by the students of Aristotelian philosophy. It is impossible, they gravely urged, that the Eye of the Universe should suffer from ophthalmia; and it is related that when Scheiner communicated his discovery of the solar spots to the provincial of his order, the latter, who was an earnest Aristotelian, answered, 'I have read Aristotle's writings from beginning to end many times, and I can assure you I have nowhere found in them anything similar to what you mention; go, therefore, my son; tranquillise yourself; be assured that what you take for spots in the Sun are the faults of your glasses or your eyes.'

Despite the defenders of Aristotle's infallibility, however, the progress of solar research went on. Galileo continued his labours, until, from viewing the Sun so often without the dark glasses now commonly employed, he lost his eyesight. Scheiner, Hevelius, and other observers, added largely to the store of known facts; and gradually the observation of solar spots began to be recognised as a regular part of the astronomer's work.

I do not propose, however, to give a detailed account of the observations made in those earlier years of telescopic observation of the Sun. Indeed, to give a full history of those observations, and to extend the

same fulness of narrative to later researches, would require twenty or thirty such volumes as the present.

I shall content myself with selecting certain illustrative instances of solar observation by the earlier astronomers, choosing those observations specially which tend to introduce the results more completely educed by recent researches.

Cassini writes thus of a spot observed by him in August 1671.* 'It is now about twenty years since, that astronomers have not seen any considerable spots on the Sun, though before that time, since the invention of the telescope, they have from time to time observed them. The Sun appeared all that while with an entire brightness, and I saw him so on the ninth of the month of August. But on the eleventh of the same, about six a clock at night, being furnisht only with a three-foot glass, I remarked in the Sun's disque two spots very dark, distant from his apparent centre about the third part of his semi-diameter. . . . The first of these spots, being look'd upon with a telescope of seventeen foot long, appeared of a somewhat oval figure; the other was oblong and a little curved, like the Hebrew letter Jod; and both together were surrounded by a *corolla* or coronet made up of little dark points' (the penumbra) 'which conformed itself to the figure of the spots, considered as they were joined together. . . . The twelfth of August, 1671, I ob-

* *New Observations of Spots in the Sun; made at the Royal Academy of Paris on August* 11, 12, and 13, 1671; *and Englisht out of the French. Phil. Trans.* vol. vi. p. 2251.

served them from the time of sun-rising, and perceived that they were nearer his centre. . . . The first was composed of two others almost round and conjoyn'd. The second represented the shape of a scorpion. The third was round' (this is the first intimation we have of the triplicity of the group), 'and they were all three environed with a coronet, which was composed, as said above, of abundance of little obscure pricks. This coronet appeared to be clearer than the rest of the Sun when looked upon with the short glass, and darker when seen with the long. Without it there were other points, but very black ones, viz. five near the round spot on the south side, and another near the scorpion's tail on the north side. At eight a clock and forty-eight minuts, the figure of the scorpion was seen divided into several pieces, as if his tail and arms had been cut off. The northern point remained no more, there remaining none but those seen on the south side, and the length of the enclosure of all the spots, comprehended between the extremities, was of one minut and fifteen seconds, and the breadth of thirty seconds.' On August 12 Cassini found no great change had taken place, but the black points outside the spot were now spread in a straight row. On the 13th 'the edge of the coronet was turned to a point on the south side.' The spot had indeed changed in that strange way which all observers of the Sun must be familiar with, the following extremity of the penumbra drawn out to a point which was so curled round as to be directed towards the preceding end of the spot.

Cassini afterwards traced the progress of this spot to the Sun's limb. He remarks, 'The apparent velocity nigh the centre was such that if it had continued the same, the spots would have arrived almost in four days to the limb of the disque; but in the hypothesis'—that they are either attached to the Sun's surface, or not far from it—'this apparent velocity was to lessen according as the spots should remove from the centre; as hath come to pass in effect. The diminution in the length of the misty crown' (a strange name for the penumbra) 'was in a manner proportionat to the diminution of the apparent velocity; since that when this crown was in the midle, and in a scituation wherein its true figure could be best seen, it appeared oblong and of the form of an human ear, its greatest diameter respecting east and west; but being nigh the limb, this same diameter seemed to shorten; and having appeared greatest in its first scituation, it appeared least in this, because it was almost in a circle that passed through the centre of the Sun, whose equal arches are by so much the more oblique by how much they approach more to the limb of his disque, and consequently appear less according to the rules of opticks; meantime the diameter that was turned from north to south apparently kept the same bigness it had near the centre, because it was in a circle almost parallel to the horizon of the Sun, which formed the representation of its limb, and whose equal arches' (for the same optical reasons) 'do not appear contracted.' It will be seen by this reasoning—which,

being interpreted, signifies that foreshortening caused the spot to appear longest in a direction square to a line from the Sun's centre—that Cassini had on this occasion come very near to the discovery afterwards made by Dr. Wilson, that the nucleus of a spot is at a lower level than the solar photosphere. For had he but noticed an excess of breadth in the penumbra nearest to the Sun's limb, the same just reasoning which he applied to the figure of the spot would have enabled him to pronounce at once respecting the relative level of the nucleus, the penumbra, and the photosphere. He gives a picture of the spot close by the Sun's limb, in which the penumbra is of equal width on the side next the centre and on that next the limb. Most probably no such peculiarity as Dr. Wilson detected existed in the case of this particular spot. In fact, so far as my own experience of the aspect of Sun-spots is concerned, spots such as the one observed by Cassini seldom exhibit the peculiarity in question in a manner which would enable an observer to theorise safely respecting the level at which nucleus, penumbra, and photosphere actually lie.

In November 1769, Dr. Wilson, of Glasgow, began the careful study of a large spot (visible to the naked eye). The results which he deduced from its changes of appearance are of great interest and importance. When he first examined the spot (November 22) it was situated not very far from the Western edge of the Sun's disc. On the next day he found that the spot

had changed in appearance. 'The penumbra, which on the previous day was equally broad on all sides of the nucleus,* was now very much contracted on the side which lay towards the centre of the disc, while the other parts retained nearly their former dimensions. On the 24th he again observed the spot. The distance from the limb was now only twenty-four seconds, and the contracted side of the penumbra had entirely vanished. The breadth of the nucleus on the same side also appeared to be more suddenly impaired than it ought to have been by the motion of the Sun across the disc.'

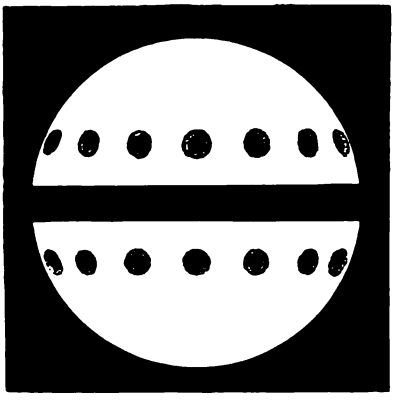
Dr. Wilson showed that these results correspond with those which would follow if the spot was a vast cavernous opening, having the nucleus at the bottom, and the penumbra forming its sloping sides. It only remained to be seen whether a corresponding succession of changes occurred when the spot re-appeared on the eastern edge, and thence passed across the solar disc. This actually happened. 'On December 11, the spot appeared on the opposite side of the disc. It was then distant about a minute and a half from the edge. The side of the penumbra next the edge, which formerly vanished, was now wholly visible, while that turned towards the centre of the disc appeared to be wanting. On December 12 it came into view, and he saw it distinctly, though narrower than the other side. He did not see the spot again until December 17, when it

* From Professor Grant's abstract of the original narrative, in his excellent *History of Physical Astronomy*.

had passed the centre of the disc, and the penumbra now appeared to surround the nucleus equally on all sides.

In fig. 41 the upper row of spots represents the succession of changes actually presented by this spot, while the lower shows what would occur as a spot traversed the Sun's disc, if the spot were simply a surface stain with a penumbral fringe. It will be seen

Fig. 41.

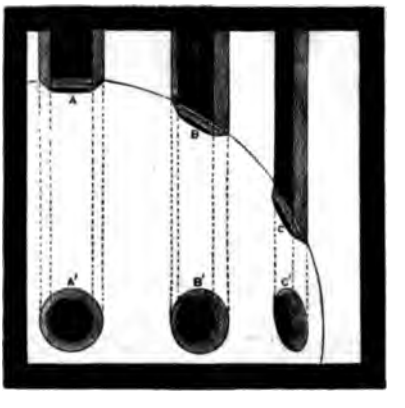


that the difference between the appearances depicted in the two rows is of a sufficiently marked character. It will also be noticed that even without any definite explanation of the peculiarities shown in the upper row, the mind at once recognises the fact that we have here to do with a cavity or depression.

Let us examine, however, the line of reasoning by which Dr. Wilson demonstrated this.

Let A, B, C (fig. 42)* be supposed to represent perspective views of a saucer-shaped depression on the surface of a sphere—the depression being all of like dimensions (the sloping sides are assumed in *these three* views to be transparent). Then, it is obvious that to an eye supposed to view them from above, the relative breadth of the black base and of the shaded sides would be indicated by the breadth of the dark and shaded spaces carried vertically upwards from the

FIG. 42.



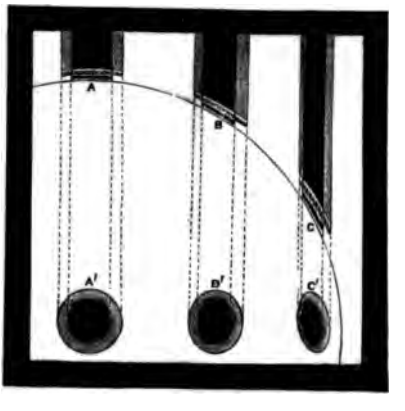
spots. Now, suppose the sphere to be rotated on a horizontal axis so that the spots are brought (their edges sliding as it were along the dotted lines) down to the positions A′, B′, C′. Then the relative breadths of the penumbra and nucleus will be indicated by the

* This figure in essentials closely resembles a figure in Fr. Secchi's recently published work on the Sun. It was drawn by me, however, and employed in illustrating lectures, nearly a year before Secchi's book appeared.

distances separating these dotted lines, and must needs therefore be such as are shown in the figure. The shape of the depression would also be as shown. We see, then, that to an eye watching a depression of this sort as it rotated from the position A′ to the position C′, changes of shape would be shown which correspond exactly with those recognised by Dr. Wilson.

This is the proper place to point out, however,

FIG. 43.



that Dr. Wilson's observations were insufficient to demonstrate that a spot is a region which lies *as a whole* beneath the solar surface. They show only that the nucleus of the spot he observed lay at a lower level than the penumbra. It is evident from fig. 43 that appearances precisely corresponding to those observed by Wilson would be seen if spots were caused by a double layer of clouds in the solar atmosphere, the lower opaque, the upper semi-transparent and ex-

tending on all sides beyond the limits covered by the lower. Such an interpretation has indeed been put forward in recent times by Kirchhoff, and is still maintained, I believe, by Spörer.

Wilson's views respecting the physical interpretation of the occurrence of these solar depressions need not detain us here. They have not been regarded as so successful as his geometrical analysis of the observed phenomena. It is only just to add that he himself did not attach equal weight to them; for in answer to objections urged by Lalande to his theory that the spots are depressions, Wilson wrote thus in 1783: 'Whether their first production and subsequent numberless changes depend upon the eructation of elastic vapours from below, or *upon eddies or whirlpools commencing at the surface*, or upon the dissolving of this luminous matter in the solar atmosphere, as clouds are melted and again given out by our air, or, if the reader pleases, upon the annihilation and reproduction of parts of this resplendent covering, is left for theory to guess at.'

Passing also over the theories of Bode, which differed in no important respect from those of Wilson, let us turn now to the observations and theories of the greatest observational astronomer the world has ever known—Sir William Herschel. I propose to treat at considerable length what he has advanced upon the subject of the Sun, partly because of the great value of his work, but partly also to re-claim for him many discoveries which have been assigned to later observers. What these are I need not in every in-

stance particularise; but it seems to me essential that the original observation of facts of so much interest and importance, only determinable by long and patient scrutiny of the Sun, should be assigned to their proper place.

Let me begin by quoting the fine passage in which Sir William Herschel speaks of the central luminary of our system.

'Among the celestial bodies,' he says, 'the Sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! it is the cause of that heat which maintains the productive power of nature, and makes the Earth a fit habitation for man! it is the central body of the planetary system; and what renders a knowledge of its nature still more interesting to us, is that the numberless stars which compose the universe, appear by the strictest analogy to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest region of space, and forcibly claims his notice.'

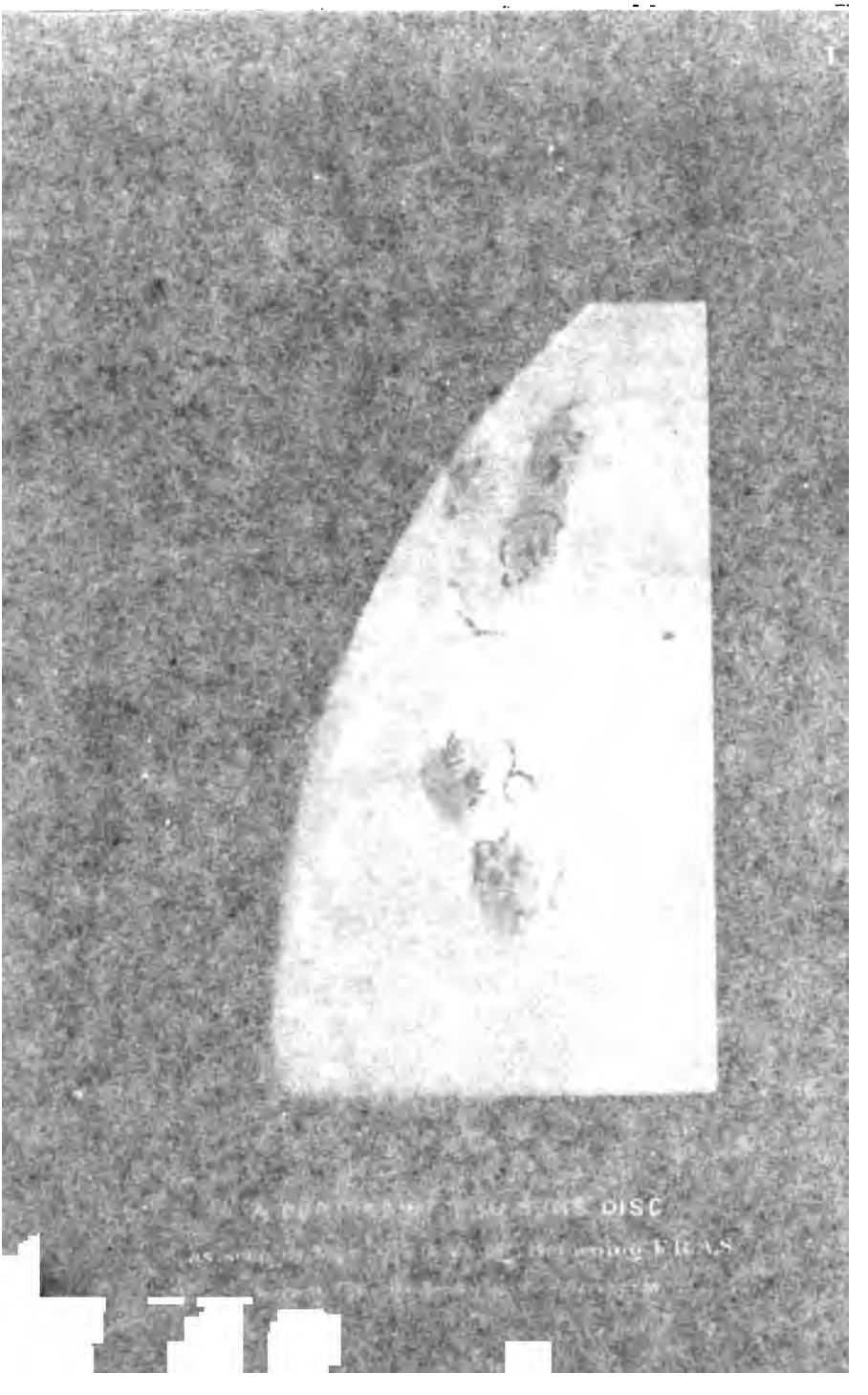
Next let us hear his summary of the theories which have been put forward respecting the physical constitution of the Sun. I may note in passing that the opening remarks are as applicable in the present day as when Herschel wrote them:—'I should not wonder,' says the great astronomer, 'if we were induced to think that nothing remained to complete our knowledge; and yet it will not be difficult to show that we are still very ignorant, at least with regard to the internal constitution of the Sun. The various conjectures

which have been formed on this subject are evident marks of the uncertainty under which we have hitherto laboured. The dark spots in the Sun, for instance, have been supposed to be solid bodies revolving very near its surface. They have been conjectured to be the smoke of volcanoes, or the scum floating upon an ocean of fluid matter. They have also been taken for clouds. They were explained to be opaque masses, swimming in the fluid matter of the Sun; dipping down occasionally. It has been supposed that a fiery liquid surrounded the Sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and that by the return of this fiery liquid they were again covered, and in that manner successively assumed different phases. The Sun itself has been called a globe of fire, though perhaps metaphorically. The waste it would undergo by a gradual consumption, on the supposition of its being ignited, has been ingeniously calculated. And in the same point of view its immense power of heating the bodies of such comets as draw very near to it has been assigned.' 'In supporting,' he proceeds, 'the ideas I shall propose in this paper with regard to the physical constitution of the Sun, I have availed myself of the labours of all these astronomers, but have been induced thereto only by my own actual observations of the solar phenomena, which, besides verifying those particulars that had been already observed, gave me such views of the solar regions as led to the foundation of a very rational

system. For having the advantage of former observation, my latest reviews of the body of the Sun were immediately directed to the most essential points; and the work was by this means facilitated and contracted into a pretty narrow compass.'

'In the year 1779,' he begins, 'there was a spot on the Sun which was large enough to be seen with the naked eye. By a view of it with a 7-feet reflector, charged with a very high power, it appeared to be divided into two parts. The largest of the two on April 19 measured 1'8"·06 in diameter, which is equal in length to more than 31,000 miles. Both together must certainly have exceeded 50,000. The idea of its being occasioned by a volcanic explosion, violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus restore the Sun, in that place, to its former splendour, ought to be rejected on many accounts. To mention only one, the great extent of the spot is very unfavourable to that supposition. Indeed, a much less violent and pernicious cause may be assigned, to account for all the appearances of the spot.

'The Earth is surrounded by an atmosphere composed of various elastic fluids. The Sun, also, has its atmosphere, and if some of the fluids which enter into its composition should be of a shining brilliancy while others are merely transparent, any temporary cause which may remove the lucid fluid, will permit us to see the body of the Sun through the transparent ones. If an observer were placed on the Moon, he would see the



solid body of our Earth only in those places where the transparent fluids of the atmosphere would permit him. In others the opaque vapours would reflect the light of the Sun without permitting his view to penetrate to the surface of our globe. He would probably also find that our planet had occasionally some shining fluids in its atmosphere; as, not unlikely, some of our northern lights might attract his notice, if they happened in the unenlightened part of the Earth, and were seen by him in his long dark night. Nay, we have pretty good reason to believe, that probably all the planets emit light in some degree; for the illumination which remains on the Moon in a total eclipse cannot be entirely ascribed to the light which may reach it by the refraction of the Earth's atmosphere.* . . In the instance of one large spot on the Sun, I concluded, from appearances, that I viewed the real solid body of the Sun itself, of which we rarely see more than its shining atmosphere.'

'In the year 1783,' he proceeds, 'I observed a fine large spot, and followed it up to the edge of the Sun's limb. Here I took notice that the spot was plainly depressed below the surface of the Sun; and that it had very broad shelving sides. I also suspected some

* There is, however, an important flaw in the reasoning on which Sir William Herschel bases this opinion. He takes the horizontal refraction due to a ray of solar light so entering the Earth's atmosphere as to reach the surface of the Earth tangentially, without noticing that the same amount of refraction will affect the ray as it passes out of the terrestrial atmosphere. This error is repeated in Ferguson's *Astronomy*, and has since been often quoted without correction.

part at least of the shelving sides to be elevated above the surface of the Sun; and observed that, contrary to what usually happens, the margin of that side of the spot which was farthest from the limb was the broadest.' It will be noticed that in the picture of the Sun presented in Plate I., Mr. Browning delineates in the case of one spot of the group a precisely analogous appearance.

Sir William Herschel's explanation of these peculiar appearances need not be quoted, as it has been disposed of by recent researches.

'In the year 1791,' he proceeds, 'I examined a large spot in the Sun, and found it evidently depressed below the level of the surface; about the third part was a broad margin or plane of considerable extent, less bright than the Sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.'*

'How very ill,' proceeds Herschel, 'would this ob-

* Herschel reasons that there could have been no deception in this appearance, because the Sun *looked* convex, whereas he had noticed that on those occasions when the Moon's mountains and valleys were apparently reversed, the Moon herself always looked concave, the illusion disappearing when the mind was directed to the fact of the Moon being in truth convex. It may be questioned, however, whether this reasoning can fairly be applied to a self-luminous body. The peculiarity affecting the apparent concavity or convexity of the lunar mountains or craters, depends entirely on the ideas present in the mind at the moment of observation, respecting the direction in which the source of illumination lies—precisely as in the analogous experiment with a seal or coin, discussed in Brewster's *Natural Magic*. Such considerations cannot affect our views respecting a self-luminous body.

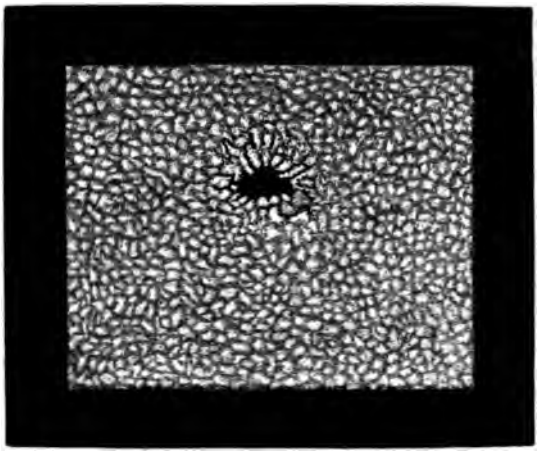
servation agree with the ideas of solid bodies bobbing up and down in a fiery liquid—with the smoke of volcanoes, or scum upon an ocean; and how easily is it explained upon our foregoing theory. The removal of the shining atmosphere, which permits us to see the Sun, must naturally be attended with a gradual diminution on its borders; an instance of a similar kind we have daily before us, when through the opening of a cloud we see the sky, which generally is attended by a surrounding haziness of some short extent, and seldom transits from a perfect clearness to its greatest obscurity.'

On August 26, 1792, Herschel examined the Sun with several powers, from 90 to 500. 'It appears evidently,' he remarks, 'that the black spots are the opaque ground or body of the Sun; and that the luminous part is an atmosphere, which being interrupted or broken, gives us a transient glimpse of the Sun itself.' He presently suggests that possibly even where there are no spots, the real surface of the Sun may now and then be perceived—'as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it; but this, I should suppose, will only happen where the lucid matter of the Sun is not very accumulated.'

A few days later he studied some well-marked faculæ. In the neighbourhood of a dark spot pretty near the edge, 'I saw,' he says, 'a great number of elevated bright places, making various figures. I shall call them *faculæ*, with Hevelius; but without assign-

ing to this term any other meaning than what it will hereafter appear ought to be given to it. I see these faculæ extended on the preceding side, over about one-sixth part of the Sun; but so far from resembling torches, they appear to me like the shrivelled elevations upon a dried apple, extended in length; and most of them are joined together, making waves, or waving lines. By some good views in the afternoon, I find

FIG. 44.



The Sun's corrugated surface.— *Secchi.*

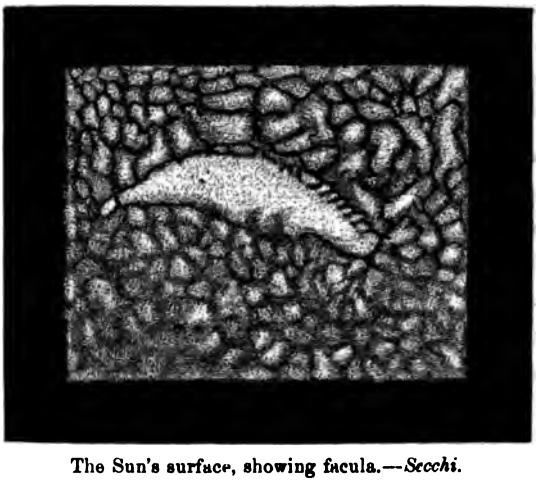
that the rest of the surface of the Sun does not contain any faculæ, except a few on the following and equatorial part of the Sun. Towards the north and south I see no faculæ; there is all over the Sun a great unevenness in the surface, which has the appearance of a mixture of small points of an unequal light; but

they are evidently an unevenness or roughness of high and low parts.'

The accompanying views (figs. 44 and 45) of portions of the Sun's surface, as delineated by Secchi with the fine telescope of the Roman observatory, correspond exactly with this description. Note also the drawing by Chacornac (fig. 55) farther on.

After a week's observation of the faculæ, Herschel thus reasons about them:—'The faculæ being eleva-

Fig. 45.



The Sun's surface, showing facula.—*Secchi.*

tions, very satisfactorily explains the reasons why they disappear towards the middle of the Sun, and reappear on the other margin; for, about the place where we lose them they begin to be edgeways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the Sun,

because they are no longer covered by the side views of these faculæ.' *

On September 22, 1792, Herschel observed few faculæ on the Sun and few spots, but the whole disc very much marked with roughness, like an orange, and some of the lowest parts of the inequalities blackish. On the following day he thus associates this roughness with the faculæ:—'The faculæ are ridges of elevation above the rough surface.' †

On February 23, 1794, Herschel noticed an appearance which is illustrated in Mr. Browning's drawing of the Sun, Plate I. 'One of the black spots on the preceding margin, which was greatly below the margin of the Sun, had, next to it, a protuberant lump of shining matter, a little brighter than the rest of the Sun. About all the spots,' he adds, 'the shining matter seems to have been disturbed; and is even lumpy and zig-zagged in an irregular manner. I call the spots black, not that they are entirely so, but merely to distinguish them; for there is not one of them to-day which is not partly or entirely covered with whitish and unequally bright nebulosity or cloudiness. This in many of them comes near to an ex-

* Perhaps the most convincing proof we have of the fact that faculæ are elevations, is that supplied by Mr. Dawes, who actually saw a facula projecting beyond the edge of the Sun's disc.

† The observation next following the above in Herschel's paper (*Phil. Trans.* vol. lxxxv.) is worth citing, though the fact involved is now well known to all observers of the Sun. It runs thus:—'Feb. 23, 1794. By an experiment I have just now tried, I find it confirmed that the Sun cannot be so distinctly viewed with a small aperture and faint darkening glasses as with a large aperture and stronger ones; this latter is the method I always use.'

tinction of the spot, and in others, seems to bring on a subdivision.'

On September 28, 1794, Sir William Herschel observed a spot similar in its general characteristics to that on the observation of which Wilson had based his hypothesis. 'There is a dark spot in the Sun on the following side. It is certainly depressed below the shining atmosphere, and has shelving sides of shining matter which rise up higher than the general surface, and are brightest at the top. The preceding shelving side is rendered almost invisible by the overhanging of the preceding elevations; while the following is very well exposed; the spot being apparently such in figure as denotes a circular form viewed in an oblique direction. Near the following margin are many bright elevations close to visible depressions. The depressed parts are less bright than the common surface. The penumbra, as it is called, about this spot is a considerable plane, of less brightness than the common surface, and seems to be as much depressed below that surface as the spot is below the plane. Hence, if the brightness of the Sun is occasioned by the lucid atmosphere, the intensity of the brightness must be less where it is depressed; for light being transparent, must be the more intense the more it is deep.'

Having thus described the most striking of his first series of observations, Herschel now proceeds to enunciate his theory respecting the solar constitution.

He remarks, in the first place, that it cannot be doubted but that the Sun has a very extensive atmo-

sphere; 'and that this atmosphere,' he proceeds, 'consists of various elastic fluids, that are more or less lucid and transparent, and of which the lucid one is that which furnishes us with light, seems also to be fully established by all the phenomena of its spots, of the faculæ, and of the lucid surface itself. There is no kind of variety in these appearances but what may be accounted for with the greatest facility, from the continued agitation which we may easily conceive must take place in the regions of such elastic fluids.'

After dwelling on certain illustrations drawn from the clouds in our own atmosphere, which Herschel (strangely enough) regards as 'probably *decompositions* of some of the elastic fluids of the atmosphere itself,' Herschel points out that the analogy of our own atmosphere will not be less to his purpose to whatever cause the clouds may owe their origin. 'The lucid clouds of the Sun, so to call them, plainly exist, because we see them; the manner of their being generated may remain an hypothesis, and mine, till a better can be proposed, may stand good; but whether it does or not, the consequences I am going to draw from what has been said will not be affected by it.'*

* It is a peculiarity of Sir William Herschel's reasoning, that it is nearly always divided very definitely into two portions, which yet many who study his writings are apt to confound. We find certain conclusions on which Sir William Herschel insists, and certain hypotheses which he simply enunciates. Respecting these last, he has perhaps as often been in the wrong as in the right, and it indicates his surprising acumen, that so even a proportion should be observed in the case of mere hypotheses. Respecting the former, I cannot recall one instance in

Herschel then states that he regards the spots as regions where the atmosphere is free from lucid clouds, the faculæ as regions where such clouds are more numerous than elsewhere. The penumbra being generally depressed about half-way between the level of the nucleus and that of the photosphere, must of course be fainter than other parts. 'No spot favourable for taking measures having lately been on the Sun,' he adds, 'I can only judge from former appearances that the regions in which the luminous solar clouds are formed, adding thereto the elevation of the faculæ, cannot be less than 1,843, nor much more than 2,765 miles in depth. It is true that in our atmosphere the extent of the clouds is limited to a very narrow compass; but we ought rather to compare the solar ones to the luminous decompositions which take place in our aurora borealis, or luminous arches, which extend much farther than the cloudy regions. The density of the luminous solar clouds, though very great, may not be exceedingly more so than that of our aurora borealis. For if we consider what would be the brilliancy of a space two or three thousand miles deep filled with such coruscations as we see now and then in our atmosphere, their apparent intensity, when

which he has been proved to have been in error. Owing to his singular clearness of mental vision, and also in part to the extreme lucidity of his descriptions, one is very apt to forget, when he is describing mere hypotheses, that he is not discussing established conclusions; and to this probably is due the fact that some of his warmest admirers do him the injustice of insisting as earnestly on views which he put forward simply as hypotheses, as though they had been enunciated by him as legitimate deductions from observed facts.

viewed at the distance of the Sun, might not be much inferior to that of the lucid solar fluid.'

From the luminous atmosphere of the Sun, Herschel proceeds to the opaque body, which he surmises to be of great solidity, on account of the power it exerts upon the planets. From the phenomena of those dark spots which have been repeatedly seen in the same place, 'and otherwise denote inequalities in their level,' he suggests that the Sun's surface 'is diversified with mountains and valleys.'

Then follows that remarkable passage which every student of astronomy knows by heart; but which yet (even though we may not accept—as I confess I do not—the opinions suggested in it) will well bear repetition:—

'The Sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or, in strictness of speaking, the only primary one of our system, all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation upon its axis, and the fall of heavy bodies, lead us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe. Whatever fanciful poets might say in making the Sun the abode of blessed spirits, or angry moralists devise in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had

any other foundations than mere opinion and vague surmise; but now I think myself authorised, *upon astronomical principles*, to propose the Sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.'

Herschel proceeds to consider the objection founded on the great heat which here at a distance of so many millions of miles we receive from the Sun, and the tremendous nature of the heat which consequently (one would suppose) must affect the imagined inhabitants of the Sun. Our admiration for the greatest astronomer of modern times must not cause us to lose sight of the fact that the reasoning at this stage of the inquiry is founded on inexact notions of the nature and laws of heat—though not such as in his day could have been unfavourably commented upon by most physicists. He remarks that the Sun's rays are 'the cause of the production of heat by uniting with the matter of fire which is contained in the substances that are heated.' He then instances the snow-covered summits of lofty mountains, and the cold experienced by aeronauts; and he concludes, 'that we have only to admit that on the Sun itself, the elastic fluids composing its atmosphere, and the matter on its surface, are of such a nature as not to be capable of any excessive affection from its own rays, which seems indeed to be proved by their copious emission.'*

* After noting other possible objections, Sir William Herschel—who did not disdain at times to be as imaginative and fanciful in theorising

In a later paper (communicated to the Royal Society in 1801) Sir William Herschel records the results of further observations. He draws special attention to certain characteristic features of the Sun's surface. These are, first, *corrugations*, which he regards as elevations and depressions causing the mottled appearance of the Sun; secondly, *nodules*, or smaller elevations in the corrugations themselves over which they are distributed as bright spots; *punctulations*, or dark spaces between the nodules; and *pores*, or 'darker coloured places in the punctulations.' He also enters into many particulars as to the behaviour of spots, pores, corrugations, nodules, and so on. To this

as he was exact and scrupulous in observing—proceeds to consider the possibility that the inhabitants of the Moon and of the satellites of Jupiter, Saturn, and Uranus, regard the primary orbs round which they travel as mere attractive centres, to keep together their orbits, to direct their revolution round the Sun, and to supply them with reflected light in the absence of direct illumination. 'Ought we not,' he asks, 'to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the Earth and those other planets that have satellites about them, perform all the offices that have been named for the inhabitants of these little globes; but to us, who live upon one of these planets, their reasonings cannot but appear very defective, when we see what a magnificent dwelling place the Earth affords to numberless intelligent beings. These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say, "The Sun (that immense globe, whose body would much more than fill the whole orbit of the Moon) is merely an attractive centre to us. From experience we can affirm that the performance of the most salutary offices to inferior planets is not inconsistent with the dignity of superior purposes; and in consequence of such analogical reasonings, assisted by telescopic views, which plainly favour the same opinion, we need not hesitate to admit that the Sun is richly stored with inhabitants."'

valuable paper, as to the other from which I have quoted, I would invite the attention of every reader interested in solar physics. Here, I shall only quote two observations bearing on the periodicity of the disturbances which affect the Sun's surface. We have seen that in 1671 Cassini had for a long time noticed the absence of Sun-spots. But on July 5, 1795, Sir William Herschel remarked that the Sun presented an appearance far more remarkable, and such, he remarks, as differed wholly 'from what he had ever seen before. There was not a single opening in the whole disc; there were no ridges or nodules, and no corrugations.' On December 9, 1798, he noticed that a similar state of things prevailed.

We may sum up as follows the views of Sir William Herschel as to the general constitution of the solar globe and surface:—He supposed the Sun to be an opaque globe surrounded by a luminous envelope. He considered that this envelope is neither fluid nor gaseous, but consists rather of luminous clouds floating in a transparent atmosphere. Beneath this layer or envelope of luminous clouds he conceived that there floats in the same atmosphere a layer of opaque clouds, rendered luminous on the outside by the light which they receive from the outer layer. These opaque clouds protect, according to this theory, the solid and relatively unilluminated nucleus of the Sun. When openings are formed in the same region in both layers of clouds, we see the body of the Sun as a dark spot. If the apertures are equally large, the spot will be

uniformly dark; but if, as more commonly happens, the outer aperture is the greater, the dark nucleus of the spot will seem to be surrounded by a dusky border. If the upper layer alone is perforated, a dusky spot without any dark central portion makes its appearance. Herschel supposed that those spots in which both layers are broken through, are caused by an uprush of some highly elastic gas breaking its way through the lower layer, and then, after expansion, removing the upper self-luminous clouds.

We shall see that while all the facts observed by Herschel have been confirmed, and while his reasoning, so far as it relates to observed facts, has been abundantly justified, some of his hypotheses have been disproved by recent observations.

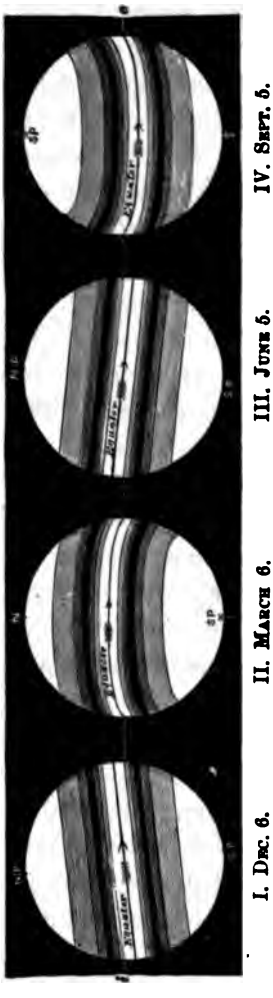
I pass on next to the researches of Sir John Herschel, recorded in that storehouse of valuable facts, the 'Results of Astronomical Observations at the South Cape.'

Sir John Herschel's observations led him to pay particular attention to a feature of the solar surface which had been first noted by Galileo. The spots are confined to two definite zones, extending about 35°* on each side of the equator; an intermediate zone to a distance of some 8° on either side of the solar equator

* If we may trust an observation of La Hire's (which, however, Mr. Carrington, than whom no higher authority can be cited, is disposed to reject), a spot has been seen as far as 70° from the Sun's equator. In 1846, Dr. Peters, of Altona, saw a spot 50° 55′ from the equator, while Carrington and Capocci have each seen spots about 45° from that circle.

being ordinarily free from spots. Fig. 46 serves to indicate the regions where spots occur, and also (where the darkest zones are shown) those regions in which spots occur most frequently and attain the greatest dimensions. The Sun is so placed in the four views as to show the way in which the spot-belts are actually presented—I. early in December; II. early in March; III. early in June; and IV. early in September. The actual dates are those indicated under the several figures. I shall have to discuss, further on, those researches of Carrington and others into the laws of the Sun's rotation and the position of the solar equator * on which these dates and the presentations shown in fig. 46 are founded.

FIG. 46.



It was to the explanation of this peculiarity that Sir John Herschel directed his chief at-

* In some works on astronomy pictures are admitted in which the effects of the Sun's inclination are altogether exaggerated.

tention. He remarks that the very existence of these zones 'at once refers the cause of spots to fluid circulations, modified, if not produced by the Sun's rotation, by reasoning of the very same kind whereby we connect our own system of trade and anti-trade winds with the Earth's rotation. Having given any exciting cause for the circulation of atmospheric fluids from the poles to the equator and back again, or *vice versâ*, the effect of rotation will necessarily be to modify those currents as our trade-winds and monsoons are modified, and to dispose all those meteorological phenomena (on a great scale) which accompany them as their visible manifestions, in zones parallel to the equator with a calm equatorial zone interposed.' Thus far, be it observed, Sir John Herschel is dealing with observed facts, and pointing to almost inevitable conclusions. He passes on (following in this respect, as in so many others, the procedure of his father) to hypothetical considerations, in dealing with the question—Whether any cause of atmospheric circulation can be found in the economy of the Sun, 'so far as we know and can comprehend it?'

He is thus led to the inquiry whether a transparent atmosphere extends beyond the luminous surface of the Sun. He mentions the deficiency of light at the borders of the visible disc of the Sun, remarking that this feature is so obvious that he is surprised it should ever have been controverted. He mentions in corroboration 'the extraordinary phenomenon of the rose-coloured solar clouds witnessed during the total eclipse

of July 8, 1842, which must have floated in, and been sustained by, an exterior transparent atmosphere.' And he suggests that this atmosphere must extend to some distance beyond the visible disc, because the darkening of the solar disc is not limited to the immediate neighbourhood of the edge, but extends some distance within the disc.*

Assuming the existence of such an atmosphere, the rotation of the Sun would cause the outer surface of the atmosphere to take up an oblately spheroidal figure, the least axis of which would correspond with the polar axis of the Sun. 'Consequently, the equatorial portions of this envelope must be of a thickness differing from that of the polar, *density for density*, so that a different obstacle must be thereby opposed to the escape of heat from the equatorial and the polar regions of the Sun. The former therefore ought, according to this reasoning, to be habitually maintained at a different temperature from the latter.' 'The spots,' adds Sir John Herschel, 'would come, on this view of the subject, to be assimilated to those regions on the Earth's surface in which, for the moment, hurricanes and tornadoes prevail—the upper stratum being tem-

* This argument, however, is not strictly sound. The extension of the darkening over the disc indicates shallowness rather than depth. This is easily seen, if we consider that were the atmosphere indefinitely deep, the luminosity of the disc would be uniform. In the actual case, of course, the atmosphere is not uniformly dense; but still the reasoning is analogous, and the extension of the darkening over the disc implies shallowness rather than the reverse; though not the same degree of shallowness as would follow in the case of an atmosphere of uniform density.

porarily carried downwards, displacing by its impetus the two strata of luminous matter beneath (which may be conceived as forming an habitually tranquil limit between the opposite upper and under currents), the upper of course to a greater extent than the lower, and thus wholly or partially denuding the opaque surface of the Sun below. Such processes cannot be unaccompanied by vorticose motions, which, left to themselves, die away by degrees and dissipate—with this peculiarity, that their lower portions come to rest more speedily than their upper, by reason of the greater resistance below, as well as the remoteness from the point of action, which lies in a higher region, so that their centre (as seen in our water-spouts, which are nothing but small tornadoes) appears to retreat upwards. Now, this agrees perfectly with what is observed during the obliteration of the solar spots, which appear as if filled in by the collapse of their sides, the penumbra closing in upon the spot, and disappearing after it.'

With all deference to one who is as high an authority in meteorological and thermological questions (which are both involved in this matter) as in astronomical matters, I must venture to point out what appears to me a flaw in the reasoning by which an excess of heat is assigned to the solar equator. If we assume that the depth of the solar atmosphere is really greater at the solar equator, then we cannot but admit as an inevitable sequel that this atmosphere, checking as it does (and as the theory itself requires), the radiation of heat from the solar equatorial regions, to a greater extent than that from the polar regions, would cause

the former regions as observed by us to appear deficient in heat-rays (and presumably in light-rays also). The deficiency must certainly become observable if the amount of heat retained in this way were adequate to produce the effects described by Sir John Herschel.

I am not endeavouring, be it understood, to negative the general conclusion to which Herschel has been led, that a difference of condition really prevails between the equatorial and polar regions to a degree sufficing to account for the spot zone as a zone of solar cyclones. Very probably this difference of temperature subsists, and almost certainly, whatever the cause may be, the spot zone is a zone of solar tornadoes. But the imagined action of a deep atmospheric layer over the solar equator seems incompatible with the observed appearance of the solar disc.

We come next to a most important series of observations directed, by Schwabe of Dessau, to the determination of the laws according to which the number and size of the solar spots may vary from time to time. Such at least was the character which Schwabe's researches eventually assumed. At first they were directed to less important objects, though from the beginning the observation of the Sun on every day when its face can be seen formed a part of Schwabe's plan.*

His observations began in the year 1826, during which many spots were visible from time to time upon

* 'I went out like Saul,' he afterwards said, 'to seek my father's asses, and lo! I found a kingdom.'

the Sun's face; indeed, on the 277 days during which the weather permitted Schwabe to observe the Sun, there were but twenty-two on which no spots could be seen. In the next year there were even more spots, and only two days when none were seen. In the next two years the Sun's face was not on any day seen without spots. In 1830 only one day occurred on which no spots could be seen; in 1831, only three. But in 1832 there were no less than forty-nine days (out of 270 on which observations were made) during which no spots were seen. In 1833 there were 139 such days out of 267; in 1834, 120 out of 273; in 1835, 18 out of 244; and then followed four years during which not a single observing day occurred on which no spots were visible.

Schwabe recognised even at this early stage of the inquiry (only twelve years having as yet elapsed since he began his researches) that a certain periodicity marks the occurrence of Sun-spots, or rather, the recurrence of years rich and poor in such phenomena. It is true that as he had noted but one full period, it might seem that he had absolutely no evidence on which to ground such a view. But in the observation of periodic variations, there are other features besides the periodic return of maxima or minima to guide the experienced observer. The progression towards and from the maximum or minimum is as instructive when carefully watched as the recurrence of many maxima. The observer requires only to assure himself that this progression possesses certain characteristics, to feel

assured that he is dealing with no accidental relations, but with true periodic changes. These characteristics are, chiefly, a steady (not uniform) progression from maximum to minimum, and *vice versâ*, a rapidity of change midway between maximum and minimum as compared with the rate of change near either of these stages, and a certain uniformity of character in the progression towards each stage (though the progression from minimum to maximum may not resemble in character the return towards the minimum). These characteristics Schwabe noted, and he felt satisfied that the numerical relations of the solar spots vary in a truly periodic manner.

But he felt that further observations were necessary to convince the scientific world—always slow to recognise new truths, or to accept results not rendered palpable, so to speak, by an accumulation of evidence. He laboured on therefore for twenty more years, tracing the gradual increase and diminution of spots in frequency and in their general dimensions, until he had completed the observation of no less than three complete oscillations from maximum frequency through minimum back to maximum again. It began to be felt that Schwabe was establishing his case, and accordingly the scientific societies of Europe (somewhat tardily, it must be admitted) accepted the result which Schwabe had demonstrated,—viz., that the solar spots increase and diminish in frequency and size in a period of about ten years. As well remarked by Mr. Johnson, of the Royal Astronomical Society :—

'Herr Schwabe displayed untiring energy in bringing his researches to a successful issue. Twelve years he spent to satisfy himself; six more years to satisfy, and still thirteen more to convince mankind. For thirty years never has the Sun exhibited his disc above the horizon of Dessau without being confronted by Schwabe's imperturbable telescope, and that appears to have happened on an average about 300 days a year. So that supposing he observed but once a day, he has made 9,000 observations, in the course of which he discovered 4,700 groups. This is, I believe, an instance of devoted persistence (if the word were not equivocal, I should say pertinacity) unsurpassed in the annals of astronomy. The energy of one man has revealed a phenomenon which had eluded even the suspicion of astronomers for 200 years.'

But even after these thirty-one years of labour, Schwabe continued his observations. With the exception of a few weeks during which he was unwell, he watched the Sun as pertinaciously as ever until a few months before his death. The results of his labours (so far as the question of the periodicity of spot-frequency is concerned) are included in the table on p. 198.

The careful study of Schwabe's results (combined with such scattered records as the observations of former astronomers supply) has led Professor Wolf, of Zürich, to the conclusion that a period of 11·11 years (or the ninth part of a century) is indicated, rather than a ten-yearly period. He also recognises

the existence of minor periods. 'He finds,' says Sir John Herschel, 'that a perceptibly greater degree of apparent activity prevails annually, on the average, in the months of September to January, than in the other months of the year; and, again, by projecting all the results in a continuous curve, he finds a series of small undulations succeeding each other at an average interval of 7·65 months, or 0·637 of a year. Now, the periodic time of Venus (225^{d}), reduced to a fraction of a year, is 0·616,—a coincidence certainly near enough to warrant some considerable suspicion of a physical connection.' A long period, estimated at about fifty-six years, has also been suspected.

The most cursory examination of the numbers in the table given in the next page, suffices to indicate the peculiarity that the progression from minimum to maximum is more rapid than the progression from maximum to minimum. In other words, if we regard the periodic changes of spot-frequency in the light of a series of waves—the maxima corresponding to the crests of the waves and the minima to the 'troughs'—the front slope of each wave is more abrupt than the rear slope.

One of the most remarkable of the results following Schwabe's noble discovery was the recognition of an association between the Sun-spot period and magnetic disturbances on the Earth.

In every part of the Earth the magnetic needle has at any given epoch a certain definite position, about which, under normal conditions, it would oscillate

Year	Days of observations	Days without spots	New groups
1826	277	22	118
1827	273	2	161
1828	282	0	225
1829	244	0	199
1830	217	1	190
1831	239	3	149
1832	270	49	84
1833	247	139	33
1834	273	120	51
1835	244	18	173
1836	200	0	272
1837	168	0	333
1838	202	0	282
1839	205	0	162
1840	263	3	152
1841	283	15	102
1842	307	64	68
1843	312	149	34
1844	321	111	52
1845	332	29	114
1846	314	1	157
1847	276	0	257
1848	278	0	330
1849	285	0	238
1850	308	2	186
1851	308	0	141
1852	337	2	125
1853	299	4	91
1854	334	65	67
1855	313	146	28
1856	321	193	34
1857	324	52	98
1858	335	0	202
1859	343	0	205
1860	332	0	211
1861	322	0	204
1862	317	3	160
1863	330	2	124
1864	325	4	130
1865	307	26	93
1866	349	76	45
1867	312	195	25
1868	301	23	101
1869*	196	0	224

* This line is from the records of the Kew Observatory.

during the day. Both as regards inclination and direction with respect to the compass-points (called the magnetic declination), this position may be regarded as determinate, at least for every fixed observatory; and, further, the intensity of the needle's directive power, that is, the energy with which, if slightly disturbed, it seeks to recover its position of rest, may also be regarded as determinate. From year to year all these magnetic elements undergo change; but with these changes we are not here concerned.* Changes of a much more minute character,

* I may note here my belief that the recognition of the laws affecting the secular variation of the Earth's magnetism would be simplified if attention were primarily directed to the magnetic lines determined by the inclination of the needle, instead of to those which depend on the intensity of the directive action. Fully admitting the weight of General Sabine's arguments as to the importance of the intensity, and as to its being the more essentially magnetic element, as it were, (north and south, or vertical and horizontal, having no direct relation to the Earth's magnetic forces,) I yet cannot but regard the inclination as affording the more trustworthy means of determining the geographical features (so to speak) of terrestrial magnetism. As the matter is of some importance, I will exhibit the reasons on which I found this opinion, leaving others to judge whether they are or are not valid. The intensity, as well as the inclination and declination (which together form one magnetic feature,—the position of rest), is doubtless affected by local circumstances; so that, superadded, as it were, on the normal intensity, there is an intensity depending on local conditions; and so in like manner, as to the inclination and declination. Now, the question arises, which of these two features is likely to be most *significantly* affected by these doubtless slight peculiarities? In determining, for example, the position of the magnetic equator and poles, with reference to intensity or inclination, would the error due to some small increment or decrement of intensity cause a greater or less divergence from the true position than a correspondingly small increment or decrement of inclination? The answer is obvious. The intensity-equator is the line of minimum intensity, and the intensity poles are points of maximum intensity. Near a minimum or a maximum, quantities change very

and the changes affecting these changes, are what we have at present to deal with. Each day the needle oscillates gently about its position of rest, the oscillation corresponding to a very slight tendency on the part of that end of the needle which lies nearest to the Sun to direct itself towards his place. The daily oscillation is itself variable in a systematic manner, not only with the progress of the year, but with that of the lunar month. The daily oscillation also varies at times in a sudden and irregular manner. The needle has been exhibiting for several weeks the most perfect uniformity of oscillation. Day after day the careful observation of the needle's progress has revealed a steady swaying to and fro, such as may be seen in the

slowly, and thus a very minute increment or decrement would largely shift the estimated place of minimum or maximum intensity. But a corresponding increment or decrement of inclination would have no such effect, because the inclination changes as quickly near the inclination-equator and poles as on any inclination-latitude.

The case may fairly be compared to the determination of the geographical equator and poles. Undoubtedly gravity is a far more essentially terrestrial element than the elevation of the pole star, or of the true pole of the heavens; and also, undoubtedly, the Earth's equator is the region where gravity is least, while the poles are regions where gravity is greatest. Yet these regions are not considered sufficient to induce us to take the force of gravity as the most satisfactory indication of latitude, or to lead us to mark down as the true equator of the Earth that line along which careful observation shows that gravity has its minimum value. We know, in fact, that, however excellent the observations might be, the deduced line would differ very importantly from the true equator.

A similar objection might be urged on like grounds against the stress laid on the position of the line of no declination; since from the very nature of this line minute local peculiarities must cause enormous irregularities, and (when coupled with secular variations) the most rapid and remarkable changes of figure.

masts of a stately ship at anchor on the scarce-heaving breast of ocean. Suddenly a change is noted; irregular movements become perceptible which are totally distinct from the regular periodic oscillations. A magnetic storm is in progress, and its progress does not affect only the place of observation, but widely-extended regions of the Earth; and in some well-authenticated instances, the magnetic vibrations thrill in one moment through the whole frame of the Earth.

Lamont, of Munich, was the first to announce that these magnetic disturbances attain a maximum of frequency in periods of about ten years. This was in 1850. Two years later General Sabine and (independently) Professors Wolf and Gautier noted the coincidence of this period with that of the solar spots. Of course, mere coincidence in duration was not the sole circumstance on which they based this conclusion. It was the coincidence of maximum of spot-frequency with maximum of magnetic disturbance, and of minimum with minimum, which enabled them to assert the true correspondence of the two periods.

Wolf subsequently proved that the period of magnetic disturbances has the length of 11·11 years, which he had assigned as the correct value of the solar spot period.

A relation so strange might well excite grave doubts. Coincidences have so often misled men of science, and indeed it is so certain in the very nature of things that misleading coincidences must occur, that physicists were justified in requiring further evidence.

Such evidence fortunately was not long wanting. Independently of the continuance of observation and the close correspondence which has been observed during the past score of years between Sun-spots and magnetic phenomena, an occurrence of a very interesting nature served in 1859 to place beyond all possibility of question the influence which solar action exerts upon the Earth's magnetism. I have so often described the occurrence in my own words that I think it well in the present instance to give the words of the two telescopists by whom it was independently observed, especially as the details of the observation have recently been called in question.

On September 1, 1859, Mr. Carrington was engaged in making his customary observation of the forms and positions of the solar spots. 'I had secured diagrams,' he says, 'of all the groups and detached spots, and was engaged at the time in counting from a chronometer and recording the contacts of the spots with the cross-wires used in the observation, when within the area of the great north group (the size of which had previously excited general remark) two patches of intensely bright and white light broke out' in the middle of the group. 'My first impression was,' he adds, 'that by some chance a ray of light had penetrated a hole in the screen attached to the object glass, by which the general image is thrown into shade, for the brilliancy was fully equal to that of direct sunlight; but by at once interrupting the current observation, and causing the image to

move by turning the right ascension handle, I saw I was an unprepared witness of a very different affair. I thereupon noted down the time by the chronometer, and seeing the outbreak to be very rapidly on the increase, and being somewhat flurried by the surprise, I hastily ran to call some one to witness the exhibition with me, and on returning within 60 seconds, was mortified to find that it was already much changed and enfeebled. Very shortly afterwards the last trace was gone, and although I maintained a strict watch for nearly an hour no recurrence took place. The spots had travelled considerably from their first position, and vanished as two rapidly fading dots of white light. The instant of the first outburst was not fifteen seconds different from 11h. 18m. Greenwich mean time, and 11h. 23m. was taken for the time of disappearance. In this interval of five minutes, the two spots traversed a space of about 35,000 miles. It was impossible on first witnessing an appearance so similar to a sudden conflagration, not to expect a considerable result in the way of alteration of the details of the group in which it occurred; and I was certainly surprised, on referring to the sketch which I had carefully and satisfactorily (and I may add, fortunately) finished before the occurrence, at finding myself unable to recognise any change whatever as having taken place. The impression left upon me is that the phenomenon took place at an elevation considerably above the general surface of the Sun, and accordingly altogether above and over the great group on which it was

seen projected. Both in figure and position the patches of light seemed entirely independent of the configuration of the great spot, and of its parts, whether nucleus or umbra.'

Mr. Hodgson's account (written before he had 'exchanged any information' with Mr. Carrington) runs as follows:—'While observing a group of spots on September 1, I was suddenly surprised at the appearance of a very brilliant star of light, much brighter than the Sun's surface, most dazzling to the protected eye, illuminating the upper edges of the adjacent spots and streaks, not unlike in effect the edging of the clouds at sunset; the rays extended in all directions; and the centre might be compared to the dazzling brilliancy of the bright star Alpha Lyræ when seen in a large telescope with low power. It lasted for some five minutes, and disappeared instantaneously, about 11h. 25m. A.M.' It seems probable that whereas two spots were seen by Mr. Carrington, who observed the solar image projected on a screen, these were blended, owing to their extreme brilliancy, into the semblance of a single spot, when observed in the telescope itself by Mr. Hodgson.

At the moment when the Sun was thus disturbed, the magnetic instruments at Kew exhibited those signs which indicate the occurrence of a magnetic storm. 'It was found,' says Dr. Balfour Stewart, 'that a magnetic disturbance had broken out at the very moment when this singular appearance had been observed.' But this was not all. A magnetic storm

never rages without accompanying signs of disturbance. Auroras in both hemispheres, and the interruption of magnetic communication all over the Earth, are the signs of a great magnetic storm. Both these evidences of great disturbance were afforded during the hours which followed the solar outbreak witnessed by Carrington and Hodgson. Vivid auroras were seen not only in both hemispheres, but in latitudes where auroras are very seldom witnessed. Even in Cuba the sky was illuminated by the auroral radiance. Strong earth-currents were observed along telegraphic lines, and these currents continually changed their direction, while all the time the magnetic needles in fixed observatories were kept markedly on one side of their normal position. 'By degrees,' says Sir John Herschel,* accounts began to pour in of great auroras seen on the nights of those days, not only in these latitudes, but at Rome; in the West Indies; on the tropics within 18° of the equator (where they hardly ever appear); nay, what is still more striking, in South America and in Australia,—where, at Melbourne, on the night of September 2, the greatest aurora ever seen there made its appearance. These auroras were accompanied with unusually great electro-magnetic

* I have been careful, it will be noticed, to quote the words of many authorities in dealing with this matter. The account I gave in my *Other Worlds than Ours* has been spoken of as though it had been based on imagination; so that I have thought it well to re-examine the records and statements from which the account was really taken (though by the aid of memory instead of transcription). Although several years had elapsed since I read those statements, I do not find that my account required correction in a single essential particular.

disturbances in every part of the world. In many places the telegraph wires struck work. They had too many private messages of their own to convey. At Washington and Philadelphia, in America, the telegraphic signal-men received severe electric shocks. At a station in Norway, the telegraphic apparatus was set fire to; and at Boston, in North America, a flame of fire followed the pen of Bain's electric telegraph (which writes down the message upon chemically-prepared paper).'

It is demonstrated then that some association exists between the disturbance of the solar photosphere and the phenomena of terrestrial magnetism. What the nature of the association may be is not so clearly apparent. We have seen that the solar spot period has been supposed to be associated by Wolf with the motions of the planets, and we shall presently see that the phenomena of spots,—as their change of form and of size, the regions in which they appear, and so on,—have been conceived to depend on the planetary motions. If these views be correct, we should have to account for laws of association between the planetary motions, terrestrial magnetism, and solar spots. But, which of these three orders of phenomena should be regarded as the cause of the others; or whether the association be of the nature of cause and effect, and not rather due to some as yet unknown common cause of the two latter classes of phenomena; or whether, finally, the planets' motions, without being the direct cause of the other phenomena,

yet indirectly bring them about, remains to be determined.*

Let us next turn to Mr. Carrington's researches, intimately associated with Schwabe's, and among the most important of all the contributions which have been made in recent times to our acquaintance with solar physics.

The object to which Mr. Carrington specially devoted his energies was the endeavour to detect regularity in the distribution of the spots, the determination of the true period of rotation of the body of the Sun, and the detection of 'systematic movements or currents of the surface, if such existed in a definable manner.' In these researches he perfectly succeeded.

Carrington's series of observations, which commenced

* I would point out, however, that the consideration of the association here discussed led General Sabine, Professor Challis, and Dr. Stewart to inquire whether the coloured prominences of the Sun may not be due to solar auroras. Since we now know that the prominences are not of this nature, may we not transfer the suggestion to the solar corona and the zodiacal light? May not these be the true solar auroras? The latter, at any rate, gives the same spectrum as the aurora, and the former gives a spectrum closely analogous. If, as General Sabine suggests, an auroral outburst in the Sun 'may perhaps be responded to simultaneously by the different planets, so that the whole solar system would seem to thrill almost like a living being under the magnetic excitement,' it seems at least probable that the solar auroras would extend to distances enormously exceeding those assigned them when the prominences were taken to be such auroras. And as we have abundant evidence that terrestrial auroras occur where the atmosphere has an inconceivable rarity, it would seem possible that under the enormous action of the Sun, even the quasi-vacuum of the interplanetary spaces might be traversed by electrical discharges. The meteoric systems occupying more or less densely the whole of these spaces, would seem to afford the requisite channel for this electric action.

in the year 1853 and ended in the year 1861, shewed, in the first place, that the discrepancies between the values formerly deduced for the Sun's rotation, arise from real differences in the velocities with which the spots move in different solar latitudes. Near the equator a spot moves at a rate indicating a more rapid rotation than in higher latitudes. Further, even among spots in the same latitude, proper motions may be recognized. These latter motions are to be regarded, however, as abnormal, and simply rendering unreliable such observations as are made on but a few spots. The peculiarities affecting the motions of spots in different latitudes have been reduced by Mr. Carrington into a formula.* The following table gives the observed rates of rotation for different latitudes (the formula being based on these values):—

	Sun's rotation-period.			Rotation per day.
deg.	*d.*	*h.*	*m.*	′
50 N. Lat.	27	10	41	787
30 ,,	26	9	46	824
20 ,,	25	17	8	840
15 ,,	25	9	10	851
10 ,,	25	3	29	859
5 ,,	25	0	42	863
0 Equator	24	2	11	867
5 S. Lat.†	24	23	18	865

* This formula is as follows:—Let ξ be the angle through which a part of the Sun in latitude λ rotates in one day. Then

$$\xi = 14^\circ\,25' - 165' \sin^2 \tfrac{1}{2}\lambda.$$

Spörer gives the formula

$$\xi = 16^\circ{\cdot}8475 - 3^\circ{\cdot}3812\,(\sin\lambda + 41^\circ\,13').$$

† It is remarkable that in all southern latitudes the observed daily mean rotation is less than in the corresponding northern latitudes. It is doubtful whether we have in this relation any indication of the true

	Sun's rotation-period.			Rotation per day.
deg.	*d.*	*h.*	*m.*	′
10 S. Lat.	25	5	35	856
15 „	25	13	31	845
20 „	25	17	52	839
30 „	26	12	50	814
45 „	28	11	0	759

I shall have occasion to dwell further on upon the significance of the varying rotation-period deduced for different solar latitudes. This result of Mr. Carrington's labours cannot but be regarded as one of the most important contributions recently made to our knowledge of solar physics.

Spörer has re-examined the whole subject, taking into account later observations, and in particular those which have been made by Fr. Secchi. The following table includes the general elements of the solar rotation as deduced by Carrington and Spörer, and reduced by Secchi to the year 1869 :—

cause of the observed variations in the rate of rotation, or merely a peculiarity which would have disappeared in a long series of observations. In favour of the former view, we have the consideration that the determination for each southern as well as for each northern latitude was independently effected, so that the coincidence of the results indicates the existence of some real cause. If Sir John Herschel is right in considering that the more rapid rotation near the solar equator implies the action of external matter in maintaining the rotation of the photosphere, it may be suggested that the northern surface of the Sun being directed somewhat more fully towards that region whither the Sun's proper motion is carrying him (see the concluding chapter of this work, and the illustrative cuts), would probably be more exposed to the influence of this external action—'the frictional impulse of circulating planetary matter in process of subsidence into, and absorption by, the central body'—much as our northern hemisphere is saluted with a large number of meteoric missiles from June to December, when the northern hemisphere is in advance, than from December to June, when this hemisphere is towards the more sheltered side of the Earth.

Elements	Carrington	Spörer
Longitude of node of solar equator .	73° 57′	74° 37′
Inclination of solar equator . .	7 15	6 57
Diurnal rotation	14 18	14 26·64
Rotation-period	$25^{d}\cdot 38$	$25^{d}\cdot 2340$

(No weight can be attached to the last two decimal figures in Spörer's value of the diurnal rotation and the rotation period.) In fig. 46 the varying presentation of the Sun towards the Earth on account of the inclination of his equator to the Earth's orbit is exhibited as exactly as possible. On or about December 6,* the Earth crosses the plane of the Sun's equator (passing southwards), and then the Sun is presented as at I, fig. 46. Three months later, the Earth reaches her greatest distance south of the solar equator, so that on or about March 6 the Sun is presented as at II. On about June 5 the Earth again crosses the plane of the solar equator, this time passing northwards, and the Sun is presented as at III. Lastly, on about September 5, the Earth reaches her greatest distance north of the solar equator, and the Sun is presented as at IV.

The observations of Schwabe and Carrington have been continued, under improved conditions, by De La Rue, Stewart, and Loewy. The powers of photography,

* The date for any year can always be determined from the almanac. It is only necessary to note when the Sun's longitude is 180° + the longitude of node of Sun's equator (say 180 + 74°, or 254°). In like manner, the Earth is again in the plane of the solar equator when the Sun's longitude is about 74°. The solar longitude is given for each day in *Hannay's Almanac*, in the last column of the first page for each month.

under the able superintendence of Mr. De La Rue, have been applied to secure records of the aspect of the solar disc on every clear day. But independently of the valuable series of records thus obtained, the three physicists above-named have undertaken a careful scrutiny, not only of the solar photographs taken at Cranford and afterwards at Kew, but of the observations made by former students of the solar surface. Accumulating a vast mass of records, they have applied processes of statistical research to educe any information respecting the Sun, which, though in reality contained in observations already made, may not lie at or even near the surface.*

In the three papers by De La Rue, Stewart, and Loewy, entitled 'Researches on Solar Physics,' we find an extension and elaboration of the modes of research employed by Wilson, Schwabe, and Wolf. Where Wilson inquired into the behaviour of individual spots, our three allies discuss the peculiarities presented by hundreds of spots. Where Schwabe discussed the number of new groups of spots, they consider the area of the spotted portion of the Sun's surface. And, lastly, where Wolf examined the evidence which the numerical statistics respecting spots afford in favour of the theory that the planets exert an influence on the solar envelopes, De La Rue and his colleagues inquire into the behaviour of individual spots as they approach or

* It is a promising sign of progress when students of science are thus willing to discuss the labours of others as well as their own, undeterred by the fear of being called mere theorists.

cross the region towards which the several planets lie; they examine the general distribution of spots as respects proximity to the equator under certain circumstances of planetary position; and by discovering other like peculiarities they obtain evidence altogether distinct in character from that adduced by Professor Wolf.

Taking first the question whether the umbræ of spots are below the level of the photosphere, the inquirers examine 605 observed cases of spots having measurable penumbræ, and they find that while in seventy-five instances the penumbra is equal on both sides (referring to Dr. Wilson's mode of dealing with the question,) in 456 instances the penumbra was widest on the side nearest the limb, while in seventy-four the penumbra was narrowest on that side. Hence the percentage of favourable instances is 75·37; of unfavourable cases, 12·23; and of neutral cases, 12·40.* This seems to place the existence of a real depression beyond question; while, at the same time, it demonstrates the truth of what Carrington has said respecting Wilson's observations,—that 'there is more variety in the appearances than Wilson confesses to, and there are marked departures from his description of form, which is rather one specific type out of several which might be adduced, and will be familiar to every one when photography

* In the *Researches* the neutral instances are not taken into account, and thus the percentage of favourable cases becomes 86·04, and that of unfavourable cases 13·96. But the rejection of neutral cases is not in accordance with the accepted rules for dealing with such matters. I can discover no reason for making an exception in this particular case.

has furnished us with forms on which all, whether observers or not, may rely.' I would invite the special attention of the reader at this point to Plate I., where Mr. Browning has delineated a case in which two spots close by each other exhibit altogether different characteristics, one agreeing with Dr. Wilson's description, the other presenting an opposite peculiarity. I was much struck with the great variety observable in this respect, when I was drawing the picture which illustrates this chapter (Plate II.), though the low power I employed did not permit me to see the minuter details of the spots.*

Next, investigating the relative position of spots and their accompanying faculæ, our inquirers find that out of 1,137 spots, 584 have their faculæ either wholly or mostly on the left—that is, behind them, as respects the motion of solar rotation, 508 have the faculæ nearly equally on both sides, while only forty-five have their faculæ mostly on the right. 'The most natural explanation of this would be,' they say, 'that the faculæ of a spot have been uplifted from the very area

* The telescope I employed was a very small one, about $2\frac{1}{8}$ inches in aperture. I used an ordinary erecting eye-piece, having a power of about 26. Thus seen, the Sun appeared in the middle of a large field, and I could scarcely have believed that I should have been able to recognise the features I had seen with larger telescopes and higher powers. I had never before examined the Sun with so low a power (for four years before the day on which the design for Plate II. was drawn I had had few opportunities for observation), and it was with a sense of considerable pleasure that I found the familiar features coming clearly into view as I scrutinised the tiny image more and more searchingly. I found myself able to comprehend better than ever before how Galileo with his small telescope and low power (30) had been able to detect so many features of the Sun's surface.

occupied by the spot, and have fallen behind to the left from being thrown up into a region of greater velocity of rotation.'

Thirdly, they attack the following question:—'Is a spot, including both umbra and penumbra, a phenomenon which takes place beneath the level of the Sun's photosphere or above it?' They note in evidence on this matter, that there are many instances in which 'a bridge of luminous matter of the same apparent luminosity as the surrounding photosphere, and unaccompanied by any penumbra, appears to cross over the umbra or centre of a spot.' Detached portions of luminous matter are also seen at times to move across a spot without producing any permanent alteration. 'On these accounts,' say the inquirers, 'we are disposed to think that a spot, including both umbra and penumbra, is a phenomenon which takes place beneath the level of the brighter part of the Sun's photosphere.' Summing up the results of this portion of their researches, they express their belief that,—

1. The umbra of a spot is nearer the Sun's centre than the penumbra, or, in other words, it is at a lower level.

2. Solar faculæ, and probably also the whole photosphere, consist of solid or liquid bodies of greater or less magnitude, either slowly sinking or suspended in equilibrium in a gaseous medium.

3. A spot including both umbra and penumbra is a phenomenon which takes places beneath the level of the Sun's photosphere.

As respects the sequent series of researches by which Messrs. De La Rue, Stewart, and Loewy have endeavoured to estimate the influence of the planets upon the solar spots, it is to be remarked that the evidence adduced seems as yet not wholly decisive. They believe that it has been rendered probable that Venus exerts a special influence on the solar spots, and that the conjunctions of the planets also affect importantly the condition of the solar photosphere. There is room, in my judgment, for some doubt as to the justice of either conclusion. It should not be forgotten that the planetary system presents so many periodic relations as to render it almost certain that any observed periodic changes in the Sun's condition may be associated statistically with some period of planetary motion—sidereal, synodical, nodical, or otherwise.* There is a remark towards the close of Carrington's volume on the solar spots, which bears very significantly on this subject. After exhibiting the relation between the phenomena of the solar spots (as tabulated by Professor Wolf) and Jupiter's variations of distance, he says, that 'from the year 1770, there is a very fair agreement between maxima of frequency and maxima of Jupiter's radius vector, and between minima and minima;' . . . but 'in the two periods which precede that date there appears to be a

* I was much struck with this fact when perusing a valuable contribution by Professor Kirkwood to the subject of planetary influences on solar phenomena. His long experience in dealing with such matters enables him to exhibit relation after relation, each showing a remarkably close agreement, as respects period, with periodic solar phenomena.

total disagreement.' 'It is important,' he then adds, 'to see before us an instance in which eight consecutive cases of general, but imperfect agreement, between the variations of two physical phenomena, are shown to be insufficient to base any conclusion upon, at the same time that they powerfully stimulate further inquiry with the view of ascertaining whether the discrepancy may admit of future explanation.'

I would by no means be understood to imply, however, that I regard the conclusions of Messrs. De La Rue, Stewart, and Loewy, respecting the influences exerted by the planets on solar phenomena, as inadmissible. On the contrary, I regard them as, on the whole, the most probable yet advanced. Based as they are on observed facts, and on statistical relations, they are worthy of the most attentive consideration. They do not seem to me, however, to be by any means demonstrated.

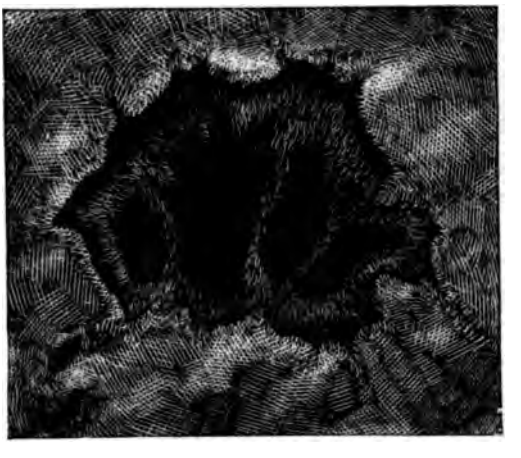
Let us now consider those features of the Sun's surface which recent observations have revealed to us.

We may begin with the famous 'willow-leaves.' It was announced by Mr. Nasmyth in 1862 that the *pores* seen in the solar photosphere are 'polygonal interstices' (I quote Sir John Herschel's account) 'between certain luminous objects of an exceedingly definite shape and general uniformity of size, whose form (at least as seen in projection in the central portions of the disc) is that of the oblong leaves of a willow-tree. These cover the whole disc of the Sun (excepting the space occupied by the spots) in countless

millions, and lie crossing each other in every imaginable direction.' The appearance of the Sun, according to this view, is exhibited in figs. 47 and 48, both of which are from drawings by Mr. Nasmyth.

This announcement led to a controversy which still remains undecided. Mr. Dawes asserted his belief that no such interlacing as Mr. Nasmyth described is

FIG. 47.



Sun-spot observed by Nasmyth, showing three bridges composed of solar willow-leaves. (*Nasmyth.*)

ever observable among the small bright spots which lie scattered over the general ground of the photosphere; that these spots can in no sense be said to resemble willow-leaves, though they present every variety of figure and size; and, lastly, that they had been long known to solar observers, and are, in fact, no other than the *nodules* of Sir William Herschel. 'The only

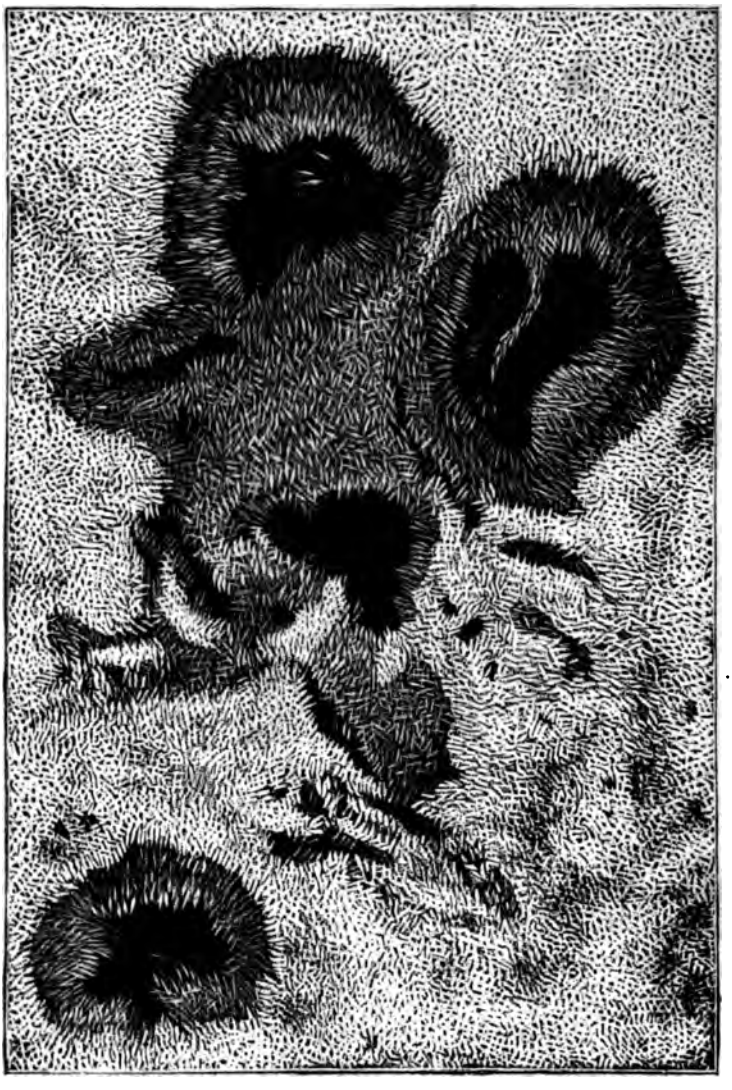
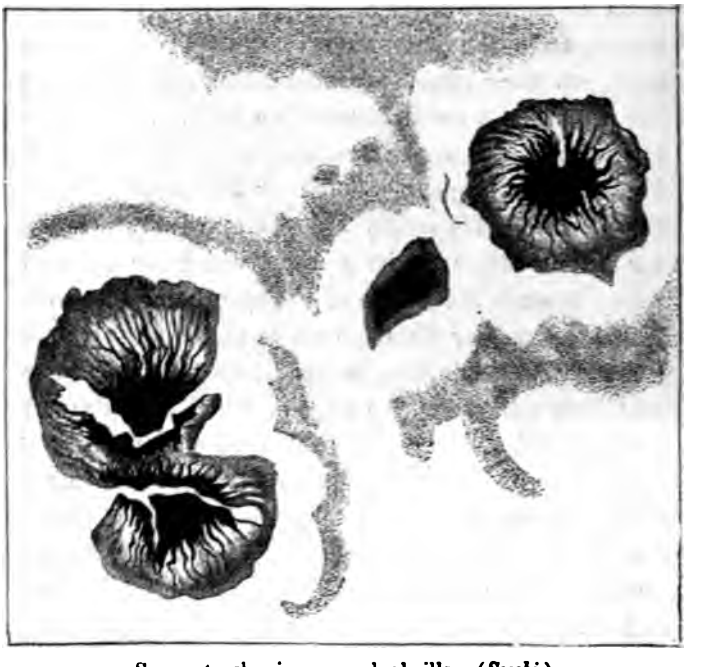


FIG. 48.—A large spot-group, showing the solar willow-leaves.

situation,' he wrote, 'in which I have usually noticed them to assume anything like the shape of willow-leaves, is in the immediate vicinity of considerable spots, on their penumbræ, and frequently projecting beyond it for a small distance on to the *umbra*,—an appearance with respect to which, in April, 1852, I used the following expressions:—"The interior edge of the penumbra frequently appears extremely jagged; the bright ridges on its surface, which are directed nearly towards the centre of the spot, being seen projected to irregular distances on to the cloudy stratum (or *umbra*), and looking much like a piece of coarse thatching with straw, the edge of which has been left untrimmed." After nearly twelve years of careful observation of the same phenomena, I do not think that I could improve upon this description.' The appearance here described will be recognised in Mr. Browning's drawing (Plate I.); it is also observable in figs. 49 and 50.

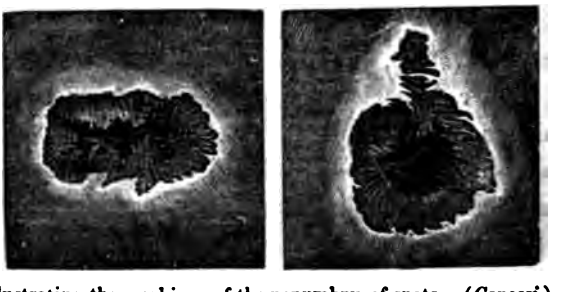
Other astronomers now joined in the discussion. Messrs. Stone and Dunkin, of Greenwich, asserted that with the fine equatorial of that observatory, luminous spots shaped like rice-grains could be seen, and that these spots, by overlapping, produce the dark pores. Fr. Secchi described the appearance of the luminous spots as resembling strokes made with a camel's-hair pencil. He described them, in fact, in terms closely according with those employed by Mr. Dawes. Dr. Huggins speaks of these luminous objects (or granules, as he calls them) as certainly not

FIG. 49.



Sun-spots, showing penumbral rills. (*Secchi.*)

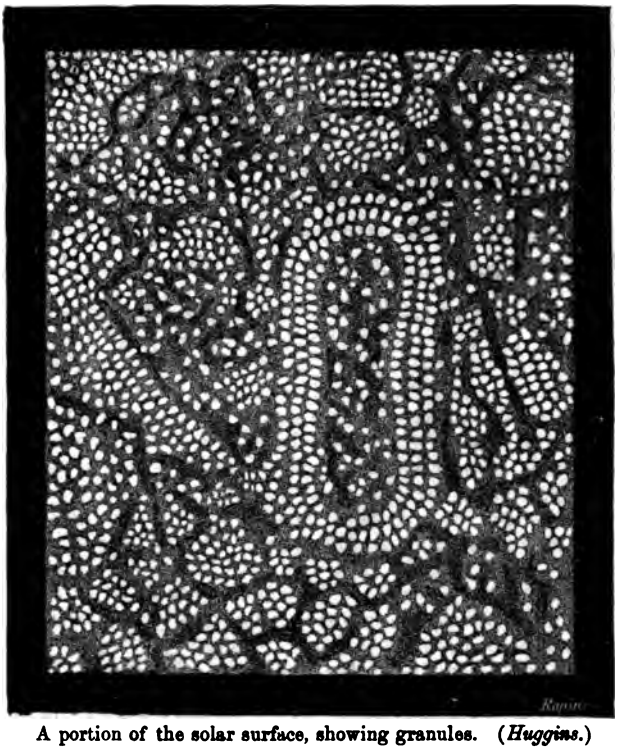
FIG. 50.



Illustrating the markings of the penumbræ of spots. (*Capocci.*)

interlacing on the general surface of the photosphere. Fig. 51 represents a view of a portion of the Sun's disc as seen by him. It will be noticed that if this view is held at a considerable distance from the eye the general

FIG. 51.



A portion of the solar surface, showing granules. (*Huggins.*)

aspect corresponds closely with the mottled appearance presented by the Sun in telescopes of moderate power.*

* In drawing Plate II. with the low power and small aperture described at p. 213, I was struck with the perfect distinctness of this mottling all over the disc.

Other observers agree with Mr. Nasmyth, who adheres to his original opinion. As regards the assertion that Sir William Herschel had long since recognised these objects, it is of course to be rejected or accepted according as Mr. Nasmyth's opinion respecting the real nature of the objects is confirmed or confuted. If there really is the interlacing he speaks of on the general surface of the Sun, then undoubtedly he is the discoverer of the phenomenon, for Sir William Herschel nowhere speaks of any such appearance (to my recollection, at least). Sir John Herschel, too, deals with Nasmyth's observation as undoubtedly new and also as of unmistakable interest and importance.*

* Let me here quote Sir John Herschel's account of Nasmyth's willow-leaf theory, partly on account of its liveliness and graphic clearness, partly for the sake of his reflections on the subject:—'These leaves, or scales, are not arranged,' he says, 'in any order (as those of a butterfly's wings are), but lie crossing one another in all directions, like what are called spills in the game of spillikins; except at the borders of a spot, where they point for the most part inwards, towards the middle of the spot, presenting much the sort of appearance that the small leaves of some water-plants or sea-weeds do at the edge of a deep hole of clear water. The exceedingly definite shape of these objects; their exact similarity one to another; and the way in which they lie across and athwart each other (except where they form a sort of bridge across a spot, in which case they seem to affect a common direction, that, namely, of the bridge itself)—all these characters seem quite repugnant to the notion of their being of a vaporous, a cloudy, or a fluid nature. Nothing remains but to consider them as separate and independent sheets, flakes, or scales, having some sort of solidity. And these flakes, be they what they may, and whatever may be said about the dashing of meteoric stones into the Sun's atmosphere, etc., are evidently the *immediate sources of the solar light and heat*, by whatever mechanism or whatever processes they may be enabled to develope, and as it were elaborate, these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view, we cannot refuse to regard them as *organisms* of some peculiar and amazing kind; and though it would be

Before leaving the subject of the willow-leaves, I must quote the latest observations bearing on this subject. On April 13, 1869, Secchi noticed that over the whole of a large spot and its neighbourhood there were multitudes of *leaves*, and that a bridge across the spot was formed of elongated leaves. The leaves in the neighbourhood of the spot were oval, the greater diameter about three times the less—thus, ⬬ 'like the leaves of the olive and certain *salices*.' He says, 'What are these things? There are veils of the most intricate structure. The spot has a third nucleus in which are the leaves seen in the drawing (fig. 52), all

FIG. 52

arranged in a radiating manner, precisely like a crystallization of sal-ammoniac seen by means of the solar

too daring to speak of such organization as partaking of the nature of life, yet we do know that vital action is competent to develope at once heat, and light, and electricity. These wonderful objects have been seen by others as well as by Mr. Nasmyth, so that there is no room to doubt of their reality. To be seen at all, however, even with the highest magnifying powers our telescopes will bear when applied to the Sun, they can hardly be less than a thousand miles in length and two or three hundred in breadth.' So that if these things are solar inhabitants whose fiery constitution enables them to illuminate, warm, and electricise the whole solar system, they are not wanting in that evidence of might which gigantic size affords. Truly, Milton's picture of him who on the fires of Hell 'lay floating many a rood,' seems tame and commonplace compared with Herschel's conception of these floating monsters, the least covering a greater space than the British Islands.

microscope—attached to a stalk (*lingua*). It is clear that the rest of the nucleus is due to the rarefaction of such leaves. There cannot be a shadow of doubt on this matter.'

On April 14, the spot in which Secchi had observed these singular objects presented the appearance depicted in fig. 53. 'It was in a marvellous condition,'

FIG. 53.

Sun-spot observed on April 14, 1869, by Secchi.

says Secchi, 'full of bridges, arcs, stalks, and leaves, like the great spot of 1866.'*

* *Memoria III. sugli spettri prismatici de' corpi celesti.* Secchi assigns the following dimensions to this remarkable group:—

Total diameter in length	2′ 37″·5
Diameter of the two principal nuclei . .	0′ 23″·78

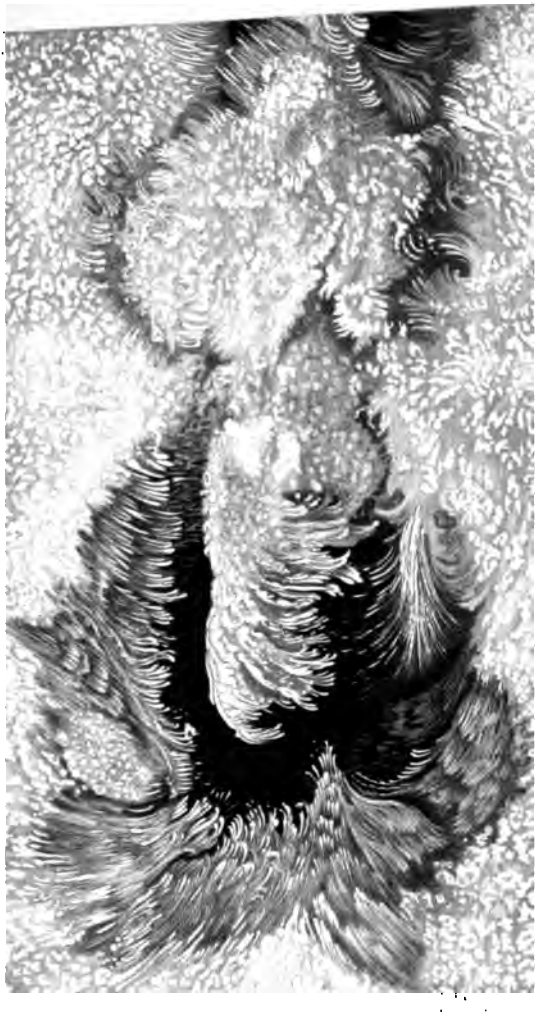
The discovery by Dawes that within the umbra, or what was formerly called the nucleus of every fully-developed spot, there exists a darker region, or (so far as telescopic research has yet gone) a true nucleus, not only opens a new field for speculation, but renders the interpretation of the phenomena before recognised altogether more difficult. By using a solar eye-piece of his own invention, in which the field of view was so contracted as to exclude even the light from the penumbræ of large spots, he detected in the stratum which had before been regarded as black, 'a mottled appearance—the degree of darkness being by no means uniform, and suggesting the idea that the surface is far from level, the lighter parts being probably the most elevated, and feebly reflecting the light received from the self-luminous strata above it. . . In all spots which are tolerably symmetrical, and large enough to admit of accurate scrutiny, this *umbra* will be found to be perforated near its centre by a *perfectly black hole*, which is to be regarded as the true nucleus.'

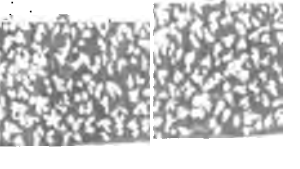
From the researches of Fr. Secchi it would appear as though the umbral portion of large spots were formed of luminous matter which is undergoing a continual process of dissipation towards its interior edge. He compares the process to the gradual dissipation of cumulus clouds under the heat of a summer Sun, and regards the umbra as of the nature of a veil

Breadth of the spot	0′ 42″·03
Breadth of the nucleus	0′ 23″·70
Breadth of the bridge	0′ 1″·50

of clouds; the nucleus as a region where an intenser heat has caused these clouds to melt away. Within spots such as these he has recognised the presence of coloured matter, which he compares to the coloured envelope whence the prominences spring; and it was partly on account of his having noted such appearances that he pronounced, so far back as 1860, his conviction that an envelope of this coloured matter—the chromatosphere of the next chapter—surrounds the whole globe of the Sun. These appearances have been noticed also by other observers. Some have recognised the gradual disappearance of portions of luminous matter (as if by subsidence in some semi-transparent medium) in the umbræ of large spots; while the presence of various tints of red within the spots has been noticed by several observers.

Professor Langley, of the Allegheny Observatory, near Pittsburg, U.S., has studied the features of the solar photosphere more closely, perhaps, than any other observer. Speaking of the 'so-called' "crystalline" shapes seen at times, and which are especially associated with large spots and periods of great disturbance,' he says, 'Doubtless owing to the difficulty of seeing appearances so delicate, these "crystalline" types have never been minutely delineated, and it has been naturally assumed that their existence lent some confirmation to the views of those who regard the photosphere as the luminous covering of an incandescent fluid, and consider spots as deposits of cooling matter, more or less analogous to the scoria deposits of





terrestrial volcanoes. These forms are so remarkable, and at first sight so apparently confirmatory of the views alluded to, that only after long study I have been led to think them not so much assimilable to the products of cooling upon a liquid surface as to certain cloud forms of our own atmosphere.

'To furnish material for a public examination of these details, whose study is so eminently instructive, it is necessary, as has been already remarked (since photography cannot yet seize them), to make drawings in which the single aim of the designer is to set down with a minute fidelity specific forms; aiming, in short, much more to produce a piece of accurate topography than a picture; but while it is on studies made of this minute exactness that discussion will be most profitable, their reproduction for the press is a work of so much labour that this kind of illustration will probably remain unusual.

'The steel engraving, Plate VIII., from studies made at the Allegheny Observatory, chiefly with the full aperture (13 English inches) of its equatorial, has been prepared by the kind furtherance of Professor George F. Barker, of Philadelphia. I trust it will be accepted as a means of putting the reader in a certain sense in the place of the observer, and enabling him himself to directly compare theory with the facts of observation. This plate is made from sketches taken on the 23rd, 24th, and 25th of December, 1873, of the eastern extremity of the great spot then nearing the centre of the Sun, and about 12° south of the solar

equator. It is called a "typical spot" because (since the details could not be completed at a sitting) it is less an accurate outline of what could be seen at any one moment than an assemblage of the different types presented, in their proper connexion. The whole, then, is taken from observation; but while the details of the adjacent photosphere have been supplemented from other studies, everything in the main body of the spot is the most literal transcript I could make of specific penumbral and umbral forms.

'The Sun had been hidden here for some days before December 23rd, when the sky cleared, disclosing a spot of more than usual size. Although a daily record of the solar surface is maintained at the Allegheny Observatory, the weather for some weeks before had interrupted it so capriciously that I am unable to say with certainty what the age of the spot was when it suddenly presented itself, but unquestionably it had at this time already passed through the initiatory stage of its formation, and had entered upon that in which the forms seen earlier have commenced to become segmented or distorted, while still retaining characteristics which show the type from which they have sprung.

'Attention was first directed to that dark interior, in which Dawes discerned still darker shades, which he called *nuclei*, as the unusual size of the spot and the irregularity of shade in the umbra seemed to favour their investigation. Aided by special optical devices, there became visible to close attention, forms which

appeared to be affiliated to the better known ones of the penumbra, which were studied also, and a description of a part of whose characteristics, interesting, perhaps, in their bearing upon solar theories, follows.

'It was observed: (1.) That the now well-known filaments of the penumbra, and those (still to be described) of the umbra, were all disposed in curves. These curves might be described in general as portions of rude spirals, since while there was such a variety as to make classification difficult, the spiral type was, as a whole, beyond any question the dominant one. This and the characteristic forms of the outer penumbral edge, elsewhere partly described, bear witness to the existence of a force, or perhaps I should say the component of a force, directed in a general sense to the centre of the spot, while at the same time the absence of a common direction of rotation, and the existence even of distinctly marked opposite flexures in the same filaments, show the complexity of the action which had been at work.

'(2.) An appearance which deserves remark is this. It has long since been observed that the interior border of the penumbra is commonly brighter than its exterior; but the hitherto unrecognised cause is here shown, in a general tendency of these singular objects, the filaments, to grow progressively brighter toward their extremities. It should be noticed that it is not only here meant that these grow brighter at the inner edge of the penumbra, but that the many filaments, not long enough to reach wholly across the penumbra,

and whose ends in this case lie partly down its slope, in every case show the same tendency, so that it is difficult to resist the impression that these extremities have a general disposition to turn upward and to appear as though lifting their points above some obscuring medium.

'(3.) In this connection we may best study the umbral forms previously referred to, about which so little has hitherto been known, owing to the darkness in which they are involved. That this darkness is only relative has been long surmised, and I have found by direct experiment that when all extraneous light is excluded, except that from the "blackest" part of the umbra, this proved to be not only intrinsically bright, but insupportably intense to the naked eye. By the optical device referred to, then, I have been able to look within the Sun to some limited extent, or farther at least below the surface than is commonly seen. Thus armed, we find that the reddish-brown masses within the umbra are resolved into filaments, analogous to the penumbral ones; like them disposed in curves, and like them apparently in planes, whose direction is usually approximately horizontal. Here also we see that these umbral filaments grow brighter toward their extremities, which *appear* as if curling upward, their ends thus occasionally furnishing that appearance of isolated bright points in the umbra which has been already observed.

'Leaving these for the moment, let us consider what was, on the whole, the most remarkable feature of the

spot: a plume-like appearance in its lower portion,* which, in connection with adjacent peculiar curves, presented forms of what has been called the "crystalline" type. The impression that agencies like those which mould the delicate crystallizations of water have been here engaged, is a natural one, and has been expressed before, the term "photospheric crystal" having apparently been used by M. Chacornac as long ago as 1853. This part of the spot, if any, would seem to justify the remark of M. Gautier, that the modifications of certain spot-forms recall rather the effects of mineral or saline deposits than that of the action of whirlwinds. They may certainly be said to remind us of such deposits, but is it by a true analogy or by superficial resemblance? If we look closer; if we increase our telescopic power, we find that filaments which elsewhere possess a scarcely sensible magnitude become here of immeasurable fineness, and lie, not so much at the sharp angles of a crystalline deposit (as they with lower powers seem to do), as like finely carded wool. We may be in doubt whether to treat these "plumes" as part of the penumbral or umbral structures, their brightness seeming to affiliate them to the former, while, on minute examination, their fibres may yet be discerned prolonged through the umbral shade and rising again (apparently) above it, in the luminous points just referred to. The resemblance seen here, then, is rather to the filamentary types found in the chromosphere, and which no one

* Seen in the lower portion of the drawing, Plate VIII.

questions are purely gaseous. There is frequently remarkable symmetry displayed in these forms, but very rarely as much as in the present example, which was the most regular I have ever seen. The balance of its parts around a central axis was almost as exact as in a sculptured ornament, and here again the regularity of certain crystalline forms is suggested. The "plume," however, whatever it may be, is evidently an integral structure, not due to the casual union of heterogeneous elements, and it was found on measurement to be approximately 20″ in length and 10″ to 12″ in width. Are we prepared to admit the existence of a body properly analogous to a "crystal," covering over ten times the area of Europe? Even on the Sun, where everything is enormous, this taxes belief. But once more, the extremely attenuated filaments of the "plume" do not appear to be in any single plane. The great length of nearly 10,000 English miles, through which they apparently extend, is a curtate distance, or that in which we see them as projected on the apparent plane of the solar disc. If I do not misinterpret the indications given by the brightening ends, they can hardly be spread upon the surface of a liquid, or upon any single surface whatever,—they bend *down* and *up*. All through the umbra are to be noted similar appearances; we seem to look down through increasing depths, but as far as vision extends, without coming to any liquid or solid floor,—always down through volumes of whirling vapour (whirling, if we judge from their forms, which are disposed as if by vortical action), and grow-

ing fainter till lost to sight at an unknown depth below the surface. Speaking, then, without reference to any hypothesis, it seems to me that the resemblance to crystalline structure (though I agree that it is striking) does not appear to be more than superficial. We have at certain rare intervals remarkable cirrous clouds in our own atmosphere, whose resemblance to these forms is equally close, and in which, I think, we may see not only a resemblance but an analogy. Some of these rarer cirrous types of our own sky, which I have studied in connection with solar forms, might, so far as external appearance went, certainly be fancied to display crystalline action as clearly as any frost-figure on a window, yet we have no difficulty in seeing that in this case the eddies of our own atmosphere have been in some way a principal cause. While recognizing the danger of pushing too far conclusions drawn from terrestrial analogy, I should then (pending a more complete study of these appearances), regard them as most nearly typified by certain cloud-forms of our own atmosphere.

'This spot presented other interesting types not here referred to. But I may mention, in support of previous observations, that in the upper portion of the penumbra, just below where a considerable part of the photosphere was islanded, the sudden and abrupt change of direction of the filaments marked the apparently unmistakable passage of one cloud stratum over another. This disposition was also marked elsewhere on the spot, whole banks of clouds moving one over

the other, so as to form a *terraced* appearance. In very many other spots, this movement of one stratum over and frequently at right angles to another has been recognised, so that after long study, I have felt justified in elsewhere formally announcing it as an ascertained fact, observed not in an isolated instance, but as a general characteristic of the solar surface, whose features are thus again assimilated to atmospheric ones in some degree like our own.

'From the preceding facts of observation, it appears that the following conclusions may be drawn:—

'(1.) Without prejudice to the important considerations on which its distinguished author has framed the theory of the existence of a liquid or viscous solar shell, it seems to me, speaking simply as an observer, that, from what has been stated of the appearance of umbral forms, such a shell or crust must, if it exist, be at a distance, below the surface of the photosphere, considerable even with reference to the dimensions we here deal with.

'(2.) It seems difficult to reconcile the bright, sharply-defined inner penumbral edge, and the regular structure discerned in the umbra, with another view, in which this umbra is a sort of stagnant pool, formed by cold vapours, or clouds, which have settled there after depressing the general surface by their weight till the penumbral slope is determined. So much of the early hypothesis of Herschel as regards the umbra as an opening (however made) *through* which we look into a non-luminous interior of the sun, extending

everywhere beneath the photosphere, seems common to views which, differing widely elsewhere, agree on this point more nearly with these observations than with the views peculiar to Fr. Secchi.*

'(3.) Finally, it seems to be little more than a summary of the facts of observation already rehearsed, to say that traces of a vortical action are found throughout the spot, and especially in the umbra. The theory which regards cyclonic or vortical action as a prominent agent in determining the forms we have studied, appears then to be in closer accordance with observation than the former.'

The processes of formation, enlargement, and disappearance of spots are well worthy of study; and although no regular law has been detected in their succession, we can yet recognise certain distinctive features ordinarily belonging to each stage of development. The formation of a spot is usually preceded by the appearance of faculæ. Then a dark point makes its appearance which increases in size, the penumbral fringe being presently recognised around it, and the distinction between the umbra and the penumbra being well defined. The same clearness of definition continues ordinarily until and after the spot has reached its greatest development. But when the spot is about to diminish there is a change in this respect. The edges seem less sharp, and an appearance is presented

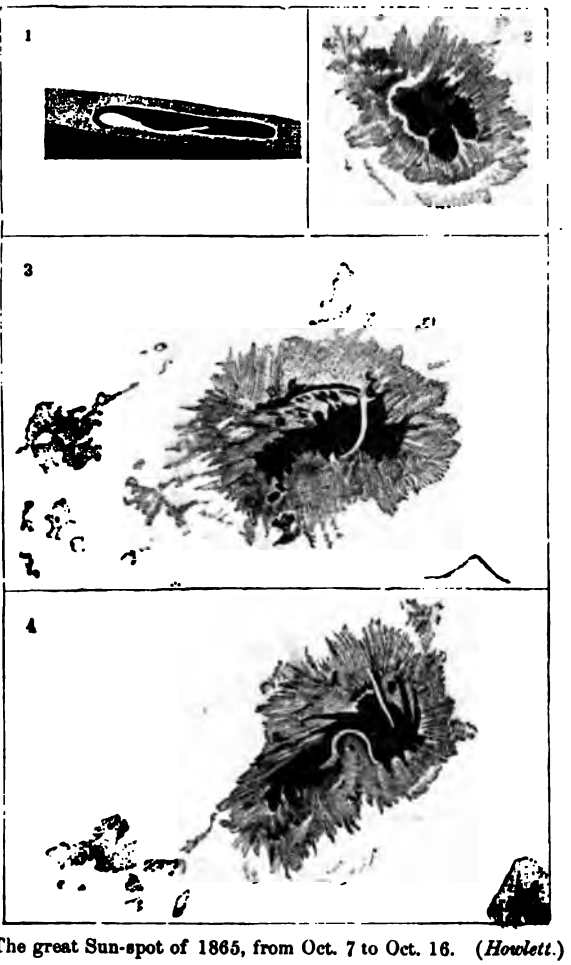
* . . . 'Il solo errore dell' illustre osservatore (i.e., Herschel) fu di estendere la massa oscura su tutto il globo sotto la fotosfera, *mentre in realtà essa non forma che pezze o chiazze assai limitate.*'—*P. Secchi, Memorie, Agosto*, 1874.

as though there floated over them a luminous cloud-veil, brighter in some places than others, and not unfrequently attaining a brightness which seems to exceed even that of the faculæ. At certain parts of the spot's circumference, this bright matter projects, hiding the whole width of the penumbra, and forming a sort of cape or promontory, with sharply serrated edges, singularly well defined against the dark background of the umbra. It is usually in this manner that the formation of a bridge begins, two promontories on opposite sides of a spot, or even on the same side, joining their extremities, so as to form either a bridge of light across the umbra, or a curved streak having both its extremities on one side of the spot. But indeed no strict law or sequence has yet been assigned to these processes of change. In a large spot the wildest and most fantastic variations will take place, and often when the spot seems approaching the stage of disappearance, it will seem to renew its existence, as though fresh forces were at work in disturbing the region it belongs to.

Some of the processes of change which take place in large spots are very well exemplified in fig. 54, where the drawings 1 to 4 show the successive changes of appearance presented by the great spot of 1865, from October 7, when it was on the Sun's eastern limb, until October 16, when it had passed the central part of the disc. These drawings were made by the Rev. Mr. Howlett, one of our most enthusiastic solar observers, and specially skilled in the delineation of Sun spots.

Were not his accuracy beyond all question, it would be

FIG. 54.

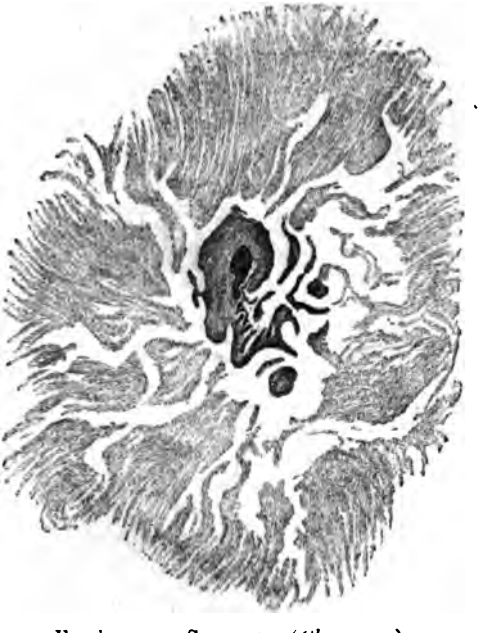


The great Sun-spot of 1865, from Oct. 7 to Oct. 16. (*Howlett.*)

worth while to note the fact that on October 16,

Chacornac made a drawing of the same spot, corresponding so closely with 4, fig. 54, that one might well suppose the two drawings were tracings from one picture.* The reader will be prepared to examine with so much the greater confidence the interesting picture of a spot with the surrounding faculæ (fig. 55),

Fig. 55.



Faculæ near a Sun-spot. (*Chacornac.*)

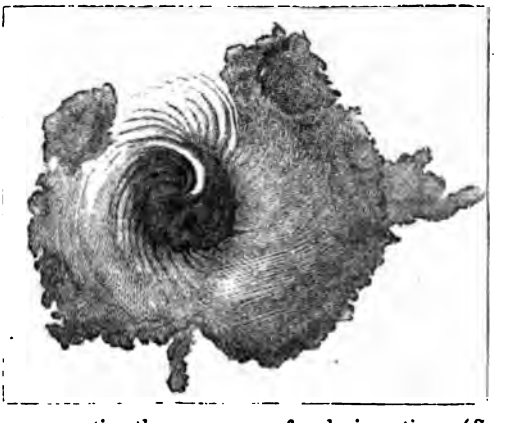
drawn by M. Chacornac. He will recognise also the close correspondence between the appearance of the

* Unfortunately such extreme accuracy of delineation is not ordinarily to be met with among solar observers.

region around this spot and Sir William Herschel's description of the faculæ (p. 177).

We owe to Mr. Dawes the detection in certain spots of a rotatory motion, as though these regions were the scene of some tremendous solar tornado. A spot of enormous dimensions was observed by him to have rotated through half a complete circuit in the

FIG. 56.



A spot presenting the appearance of cyclonic motion. (*Secchi.*)

course of about six days. Other spots have exhibited an even more rapid motion, and the spectroscopic observations made by Mr. Lockyer on parts of the Sun near the limb (where such cyclonic motions would necessarily involve a rapid motion towards and from the eye) seem to place beyond question the existence of solar tornadoes having a velocity of 40 or 50, in some cases even 120 miles per second. So that we need no longer regard (with Spörer) the whirling

appearance noticed in the accompanying drawing by Fr. Secchi (fig. 56) as due to optical illusion.

The immense dimensions of some spots well deserve thoughtful consideration. When we remember that the least spot which could be perceived with the most powerful telescope must have an area of at least 50,000 miles, it will be understood how enormous these spots must be which have been distinctly visible to the unaided eye.* But we have trustworthy measurements to refer to in this matter. Pastorff, in 1828, measured a spot whose umbra had an extent four times greater than the Earth's surface. In August, 1859, a spot was measured by Newall which had a diameter of 58,000 miles—that is, exceeding more than seven times the diameter of our Earth. But spots of even greater dimensions have been observed. In June, 1843, a spot was visible, which, according to Schwabe's measurements, had a length of no less than 74,816 miles. On March 15, 1858, observers of the great eclipse had the good fortune to witness the passage of the Moon over a spot which had a breadth of 107,520 miles. It was in the same year that the largest spot of any whose records have been handed down to us, was visible upon the solar disc. It had a breadth of more than 143,500 miles; so that across it no less than eighteen globes as large as our Earth might have been placed side by side. At a very moderate computation of the depth of this solar cavity, it may

* Of the groups shown in Plate II. three were visible to the naked eye, the largest of them (near the centre) being quite conspicuous.

be assumed that the mass of 100 earths such as ours would barely have sufficed to fill it to the level of the solar photosphere.

During the years 1870–73 many spots and groups of enormous extent were noticed. Those shown in Plate II. may be cited as instances; but others fully as large were observed.

The rapidity with which some spots have changed in figure, or even wholly disappeared, would be wholly incredible were it not that astronomers of the highest repute for accuracy have supplied the records of such changes. Dr. Wollaston says:—'I once saw with a 12-inch reflector a spot which burst in pieces as I was looking at it. I could not expect such an event, and therefore cannot be certain of the exact particulars; but the appearance, as it struck me at the time, was like that of a piece of ice when dashed on a frozen pond, which breaks in pieces and slides on the surface in various directions.'* Biela also notes that spots disappear sometimes almost in a single moment. The converse of such a change has been witnessed by Krone, who observed a spot of no inconsiderable dimensions which sprang into existence in less than a minute of time. On one occasion a momentary distraction caused Sir William Herschel to turn away his

* Of course this description refers only to the appearance which the spots ordinarily present, of being real bodies rather than openings. What Wollaston has described as the breaking up of a spot into pieces must in reality be looked upon in all probability as the sudden change of a single whirlstorm into a number of smaller ones.

eyes from a group of spots he was observing. When he looked again the group had vanished!

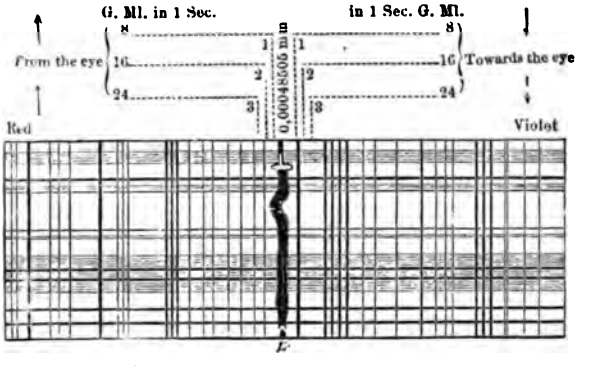
In this place I shall not enter into the discussion of the nature of the spots—a matter, indeed, on which at present it is very difficult to form an opinion. It has been my purpose in this chapter to consider rather the evidence which has been adduced respecting the solar surface than the physical theories put forward in explanation of that evidence.

Two points remain, however, to be briefly touched upon.

The first is the evidence supplied by the spectroscope respecting the spots. It is evident that by bringing a spot under the slit of the spectroscope in the way described at the close of the preceding chapter (see fig. 35) it becomes possible to institute a direct comparison between the spectrum of the umbra, penumbra, and surrounding faculæ. If any lines belonging to the ordinary solar spectrum disappear in these regions, or if new lines make their appearance, we can at once become cognisant of the fact, because we see the spectra of these regions simultaneously. In like manner we can determine whether any change takes place in the character and appearance of any solar line—for instance, whether it is wider or narrower in the spectra of certain regions, or whether it changes into a bright line. Now, fig. 57 will illustrate the peculiarities which make their appearance when a spot is brought in this way under examination. Here the length of the spectrum (only

a small portion of which is shown in the figure) is horizontal, so that the vertical lines are the dark lines of the spectrum. The horizontal lines indicate the regions of the spectrum corresponding to those parts of a spot where a general absorption takes place. It will be seen that where this general absorption is sufficient only to produce a degradation of brilliancy, all the lines in this part of the spectrum are visible. The

FIG. 57.



Illustrating the changes in certain lines in the spectra of Sun-spots.

F line (belonging to hydrogen) is, however, peculiarly affected across the whole region of the spot. At the upper and lower extremity we see it of its normal width, while over all the remaining breadth of the spectrum, except two small portions, it is much broader, and has shaded edges. In one place it is bent; along another part of its length a narrow line of light is seen to be almost centrally placed upon it; and lastly in two places it appears bright and irregularly shaped.

Now, the facts here noticed are to be thus interpreted:—Beginning with the top of the line F in fig. 57, we have first the normal black line showing that in the part of the Sun included within the uppermost part of the slit, the hydrogen is in its ordinary condition as respects temperature. It is less heated than the matter whence the main portion of the solar light is radiated, and so absorbs that portion of the light which it has itself the power of radiating. Next we come to a bright hydrogen line of the normal width on a shaded background. The corresponding part of the Sun (that is the part next below the former within the slit) is, then, either somewhat reduced in temperature, or else partially covered by generally absorbing matter, over which there is a layer of hydrogen at the normal pressure, but more heated than the radiating region below. Hence in this place the hydrogen radiates more light than it absorbs, and the line F is rendered relatively bright. Next is a region where the hydrogen line is still bright but very much wider. Over the corresponding portion of the Sun, therefore, the hydrogen not only exists at a higher temperature but at a greater pressure. Then we come to the widened dark line, indicating that over the corresponding portion of the Sun there is hydrogen at a relatively low temperature and at an abnormally high pressure. The bend towards the red end of the spectrum indicates that the corresponding portion of the hydrogen envelope is moving from the eye,* or, in other words, that there is

* The vertical dotted lines 1, 2, 3, on either side of F indicate how

in this part of the Sun a downrush of hydrogen. Where we see a relatively bright line superposed on the relatively dark one, we learn that above the compressed hydrogen at a relatively low temperature there is a layer (or tongue, or prominence) of more heated hydrogen. While, lastly, where we see the pointed dark line close to the bottom of the figure, we learn that above hydrogen as heated as the general radiating substance of the Sun, there is the usual layer of hydrogen at lower temperature, very shallow where the line is pointed, but deepening within a short distance to its normal condition.

Along a narrow strip, then, crossing the width of a solar spot, all these varieties of condition are thus recognisable. Nor is hydrogen the only element whose lines exhibit such peculiarities. The lines of sodium, magnesium, barium, and other elements, have been observed to exhibit similar indications of violent action, rapid motion, and remarkable changes of pressure. But perhaps the most striking of all the phenomena revealed by the spectroscope is the occurrence, in the spectra, of large spots, of lines and bands corresponding to those due to the presence of aqueous vapour in our own atmosphere. Fr. Secchi not only testifies to this, but he describes experiments by which he convinced himself that these lines really belong to the spots, and

far the line F should be shifted to indicate a velocity of 8, 16, and 24 geographical miles respectively from or towards the eye. The decimal figures between the vertical lines numbered 1, 1, indicate the length of the light-waves (in parts of a millimetre), corresponding to the part of the spectrum where the line F is.

not to our own atmosphere. He found that these 'water-lines' were not visible when the instrument was directed in clear weather to unspotted parts of the solar disc; but that as the instrument was shifted, the approach of a spot was clearly indicated by the appearance and gradually intensifying of these lines. When the sky was covered with thin clouds he saw the same lines towards whatever part of the Sun the instrument was directed; but they always appeared strongest in the spectrum of a spot.*

The second point which I wish to notice, in conclusion, is the evidence of the polariscope respecting the general condition of the solar photosphere. I do not feel justified in giving space to an account of the principles on which polariscopic analysis depends, because, to say the truth, the polariscope has thrown but little light on the subject of solar physics. I therefore merely state that light under certain conditions of emission, reflection, and refraction, acquires a peculiar property called polarisation, by which its capability for subsequent reflection or refraction is materially modified. We have here to deal with emission; and the special law which concerns us is this, that light emitted from an incandescent solid or liquid at a very oblique angle is partially polarised in such sort that when incident on a plane at right angles with the angle of emanation, the polarised portion does not, like the rest, undergo reflection. Now, the light from near

* The reader is referred for fuller details than there is here space for to Dr. Schellen's work *Die Spectralanalyse*, already referred to.

the edge of the Sun's disc shows no signs of having this particular quality. Hence, Arago and others have concluded that the solar photosphere cannot be formed of incandescent solid or liquid substance, but must necessarily be gaseous. We shall have occasion further on to consider the bearing of this evidence on the views we are to form respecting the Sun's physical constitution. It is necessary to note, however, that Sir John Herschel has called in question Arago's conclusion; and, without asserting that the solar photosphere must necessarily be either solid or liquid, he has shown that the evidence of the polariscope is more than questionable, since the Sun can by no means be regarded as a smooth, uniform globe. Its surface is, in all probability, so rough and uneven that the light received from near the edge may come for the most part from surfaces nearly at right angles to the visual line.*

Here I conclude my survey of the solar surface. I have presented but such portions of the vast mass of material really available as seemed most instructive and to bear most pertinently on the views we are to form respecting the Sun's physical condition. Ten or twelve such volumes as the present would be needed

* The case may be compared with that of the Moon. If the Moon were a smooth, uniform globe, she ought, when full, to seem much darker near the edge than near the centre of her disc. That she does not is due to the inequalities of her surface; and Dr. Zöllner has been able to show from the observed luminosity of the Moon at different times that the probable average inclination of the lunar mountains is about 56 degrees.

to contain even an abstract of the observations which have been made on the great central luminary of our system. To the student of solar physics I cannot too earnestly recommend the careful study of all such observations as he can obtain access to; but it has been necessary for my present purpose to give a more general view of the subject. This chapter must be regarded as bearing the same relation to the ponderous volumes in which our Carringtons and Herschels, our Schwabes and Spörers and Secchis, have recorded their observations, as Plate II. bears to Plate I., or, generally, to such large-scale and elaborate views as are afforded by powerful telescopes. We must now pass on to other matters well worthy of attention—to matters that are perhaps even more interesting and instructive than the facts which astronomers have discovered in their survey of the solar surface.

CHAPTER V.

THE PROMINENCES AND THE SIERRA.

THE coloured prominences which have recently attracted so large a share of the attention of solar physicists were first fully recognised during the total solar eclipse of 1842. It is probable, however, that they were seen more than a century before that date, though their real nature was not suspected. During the total solar eclipse of May 2, 1733, Vassenius, at Göttenburg, observed several red clouds floating, as he supposed, in the atmosphere. One of them seemed larger than the rest, and appeared to be composed of three masses placed one above the other, and completely detached from the Moon's limb. 'These spots seemed,' he writes, 'composed in each instance of three smaller parts or cloudy patches of unequal length, having a certain degree of obliquity to the Moon's periphery. Having directed the attention of my companion, who had the eyes of a lynx, to the phenomenon, I drew a sketch of its aspect. But while he, not being accustomed to the use of the telescope, was unable to find the Moon, I again, with great delight,

perceived the same spot, or, if you choose, rather the invariable cloud occupying its former situation in the atmosphere near the Moon's periphery.' We need not be surprised that Vassenius assigns the spots without scruple to the Moon's atmosphere, since it was thought by many in his time that the Moon has an atmosphere of appreciable extent. Yet it was unfortunate for science that the prominences (for we can scarcely doubt that the appearances seen by Vassenius were really prominences) should have been thus explained away as relatively unimportant phenomena, since otherwise observers during succeeding eclipses would probably have searched for similar objects, and we might thus have possessed a long series of observations tending to indicate the laws according to which these objects make their appearance.

For more than a century eclipse passed after eclipse, and no observer recognised these flames of coloured light, which have seemed to the observers of recent eclipses so striking and obvious. Ferrer, indeed, in 1806, and Van Swinden, in 1820, noticed faint traces of some peculiar coloured appendages; but their observations were not satisfactory, nor was any attention drawn to the subject.

During the great eclipse of 1842, however, a number of first-rate observers were distributed along the line of total obscuration. Airy, Arago, and the younger Struve; Littrow, Baily, Santini, Valz, and Biela—a host, in fine, of the most skilful astronomers in Europe—watched the eclipse with careful scrutiny. All of them

recognised with surprise the presence of rose-coloured prominences round the disc of the eclipsed Sun.

The Astronomer-Royal saw three prominences at the summit of the disc; Arago, Struve, and Schidlowski saw two near the lowest point of the disc; Schumacher, of Vienna, saw three—two below, and one above.

It will be instructive to consider the account given by the first observers of these interesting objects.

The Astronomer-Royal compared them to the inclined teeth in a circular saw, and estimated their height at about one minute of arc.

Schumacher compared the protuberances to icebergs, and the pictures which illustrate his paper represent them as much more closely resembling icebergs than any protuberances seen in recent times. We may not unfairly conclude that Schumacher's drawings are somewhat idealised.

Baily compared the prominences to Alpine peaks coloured by a setting Sun. He noticed that one was bifurcated almost to its base. M. Mauvais employs a similar comparison. He had seen a reddish point soon after the Sun was totally obscured. 'When fifty-six seconds had passed after the commencement of totality,' he writes, 'this reddish point transformed itself into two protuberances, resembling two adjacent mountains, and well defined. Their colour was not uniform, streaks of a deeper red marking their flanks. I cannot describe them better than by comparing them to distant Alpine peaks, illuminated by the rays of the

setting Sun. One minute and ten seconds from the time of total obscuration a third mountain became visible to the left of the other two. In colour it resembled the others. Beside it were some smaller peaks, all of them well defined.'

Mauvais noticed that the other two protuberances grew higher while the third was making its appearance. Near the end of the eclipse they were no less than two minutes of arc in height.

Biela, Schumacher, and others recognised a border of rose-coloured light surrounding a part of the Moon's limb at a lower level than that attained by the prominences. It is worthy of note that this phenomenon had been noticed earlier than the prominences themselves; for during the total eclipse of 1706, Captain Stannyan remarked that a blood-coloured streak of light appeared, before the Sun's limb emerged from behind the Moon. In 1715, also, Halley noticed that two or three seconds before the emersion, the Moon's limb appeared to be tinged with a dusky but strong, red light, forming a long and narrow streak; and during the same eclipse, Louville saw what he describes as an arc of deep red colour along the edge of the Moon's disc. The latter astronomer was careful to assure himself that the appearance was no illusion, and to this end he brought the red arc into the middle of the telescopic field of view, when he found that the red colour remained unchanged. Don Ulloa, in 1778, and Ferrer, in 1806, had noticed a similar phenomenon.

When the various accounts of the eclipse of 1842 came before the astronomical world, several theories were propounded in explanation of the red prominences. The theory that they are mountains in the Sun was for a while in favour; but Arago pointed out that some of them were too considerably inclined to the perpendicular to be so regarded. Others supposed them to be clouds in the solar atmosphere; while others, again, suspected them to be enormous flames As ordinarily happens in such cases, there were not wanting those who denied that the coloured prominences had any real existence whatever. M. Faye, for example, asserted his belief that they are purely optical illusions—'mirages, perhaps, produced near the Moon's surface.'

The eclipse of 1851 removed these doubts for the most part, though it is to be noted in passing that despite the evidence obtained then, and yet again in 1860, there were some who continued, even until the great Indian eclipse of 1868, to deny that the coloured prominences and the rose-tinted arcs seen at a lower level could really be regarded as solar appendages.

During the total eclipse of 1851 many observers of great skill made drawings of the very remarkable prominences which were visible on that occasion. These pictures exhibited a sufficiently satisfactory agreement to convince the observers that they had all witnessed the same phenomena; though the discrepancies between the pictures afford instructive evidence of the difficulty of delineating with exactness the details presented

during eclipses. The following six pictures represent in order the work of the Astronomer-Royal, Mr. Dawes, Mr. Hind, Mr. Lassell, Mr. Gray, and Mr. Stephenson. Mr. Airy thus writes respecting the prominences:—

'The form of the prominences was most remarkable. That which I have marked *a* (fig. 58) reminded me of

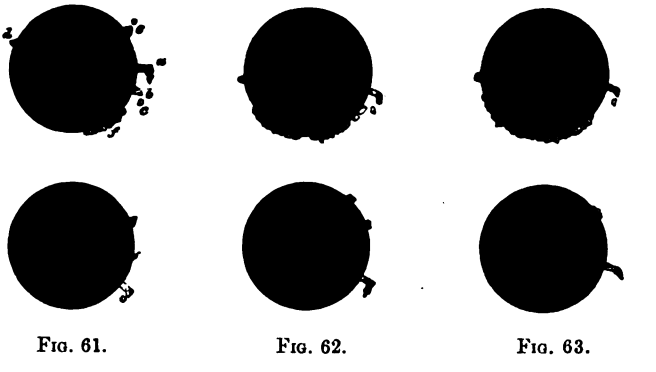


Fig. 58. Fig. 59. Fig. 60.

Fig. 61. Fig. 62. Fig. 63.

The coloured prominences seen during the eclipse of 1851.

a boomerang. Its colour for at least two-thirds of its breadth—from the convexity towards the concavity—was full lake-red; the remainder was nearly white. The most brilliant part of it was the swell farthest from the Moon's limb; this was distinctly seen by myself and my friends with the naked eye. I did not measure its height; but judging generally by its proportion to the Moon's diameter, it must have been three minutes of arc. This estimation perhaps belongs to a later

period of the eclipse. The prominence *b* was a pale white semicircle based on the Moon's limb. That marked *c* was a red detached cloud or balloon of nearly circular form, separated from the Moon's limb by a space differing in no way from the rest of the corona of nearly its own breadth. That marked *d* was a small triangular, or conical, red mountain, perhaps a little white in the interior. These were the appearances seen instantly after the formation of the totality. I employed myself in an attempt to delineate roughly the appearances on the western limb, and I took a hasty view of the country; I then remarked the Moon a second time. I believe (but I did not carefully remark) that the prominences *a b c* had increased in height; but *d* had now disappeared, and a new one, *e*, had risen up. It was impossible to see this change without feeling the conviction that the prominences belonged to the Sun, and to the Moon. I again looked around, when I saw a scene of unexpected beauty. I went to my telescope with a hope that I might be able to make the polarization observations, when I saw that the *sierra*, or rugged line of projections shown at *f*, had arisen. The *sierra* was more brilliant than the other prominences, and its colour was nearly scarlet. The other prominences had perhaps increased in height, but no additional new ones had arisen. The appearance of the *sierra* nearly in the place where I had expected the appearance of the Sun warned me not now to attempt any other physical observations. In a short time the white Sun

burst forth, and the corona and every prominence vanished.'

Mr. Hind's narrative is as follows:—'On first viewing the Sun without the dark glass after the commencement of totality, three rose-coloured prominences immediately caught my eye, and others were seen a few seconds later. The largest and most remarkable of them (*a* in Mr. Airy's drawing) was straight through two-thirds of its length, but curved like a sabre near the extremity, the concave edge being towards the horizon. The edges were of a full rose-colour, the central parts paler, though still pink. Twenty seconds, or thereabouts, after the disappearance of the Sun, I estimated its length at forty-five seconds of arc, and on attentively watching it towards the end of totality I saw it materially lengthened—probably to two minutes—the Moon having apparently left more and more of it visible as she travelled across the Sun. It was always curved, and I did not remark any change of form, nor the slightest motion during the time the Sun was hidden. I saw this extraordinary prominence *four seconds after the end of totality*, but at this time it appeared detached from the Sun's limb, the strong white light of the corona intervening between the limb and the base of the prominence. About ten degrees south of the above object I saw during the totality a detached triangular spot of the same rose-colour, suspended, as it were, in the light of the corona, which gradually receded from the Moon's dark limb, as she moved onwards, and was therefore clearly connected

with the Sun. Its form and position with respect to the large prominence continued exactly the same so long as I observed it. On the south limb of the Moon appeared a long range of rose-coloured flames, which seemed to be affected with a tremulous motion, though not to any great extent. The bright rose-red of the tops of these projections gradually faded towards their bases, and along the Moon's limb appeared a bright narrow line of a deep violet tint; not far from the western extremity of this long range of red flames was an isolated prominence, about forty seconds in altitude, and another of similar size and form at an angle of 145° from the north towards the east.'

I may add Mr. Dawes' account of the great prominence marked *a* in Mr. Airy's picture (fig. 58). 'A red protuberance of vivid brightness and very deep tint arose to a height perhaps 1½′ when first seen, and increased in length to 2′ or more, as the Moon's progress revealed it more completely. In shape it somewhat resembled a Turkish scimitar, the northern edge being convex, and the southern concave. Towards the apex it bent suddenly to the south,—or upwards as seen in the telescope. Its northern edge was well defined, and of a deeper colour than the rest, especially towards its base. I should call it a rich carmine. The southern edge was less distinctly defined, and decidedly paler. It gave me the impression of a somewhat conical protuberance, partly hidden on its southern side by some intervening substance of a soft or flocculent character. The apex of this protuberance

[illegible]

[illegible]

[illegible]

mers. All agreed as to the principal details, and, as will be seen by figs. 58–63, there was a very satisfactory agreement in the pictures taken by different observers. It would appear that no doubt could any longer remain that the prominences were solar appendages of some sort. They had been visibly traversed by the Moon, according to the unexceptionable evidence of such astronomers as Airy, Hind, Dawes, and others. They had continued visible when our atmosphere had already begun to be lighted up by the direct rays of the returning Sun. At stations far apart they had presented the same appearance. It would seem therefore that nothing was wanting to establish their real relation to the solar orb, and that no question should any longer have existed as to the fact that the prominences are true solar appendages, since the proofs were so complete that they belonged neither to the Moon nor to our own atmosphere, and, further, that they were not mere optical illusions.

Yet, in the face of all this evidence, some astronomers were still found who maintained that the observations were insufficient to establish the existence of coloured objects so enormous as these must be if they really were solar appendages. It has always happened that in the ranks of the scientific army some have been found who refuse to credit the marvels which observation is continually revealing on every hand. Despite all the known wonders of the universe, the mere circumstance that the sole available interpretation of observed facts involves some surprising conclusion, is

held by such men to be a sufficient reason for rejecting the observations of the most trustworthy astronomers.*

So it was in this instance. For nine long years astronomy was compelled to wait before she could be allowed to take possession of her well-won new territory. The amazing fact had been proved beyond all possibility of reasonable question that the great globe of the Sun is surrounded by a deep layer of coloured matters; while from portions of this vast envelope enormous protuberances start out, their height so vast

* One is almost ready to despair of the cause of scientific progress—to despair at least that that progress will ever be so rapid as it might readily become—when one finds that each new result must be established over and over again before it is admitted by a large proportion of the scientific world. It may be remarked, indeed, that the progress of science has been at least as seriously checked by undue caution as by undue boldness. It would seem almost as though some students of science were continually in dread lest the work of our observers should become too productive. The value of scientific observation seems to be enhanced in their eyes precisely in proportion as its fruits are insignificant. In all ages there have been those who would thus unwisely restrain the progress of legitimate inquiry. 'We must not admit that Jupiter has moons,' they said of old; 'the Evil One may have sent these appearances to deceive us. Let us wait for further observation.' 'The Sun cannot have spots,' they reasoned again, 'for the Eye of the Universe cannot suffer from ophthalmia. These things are illusions; let us wait for more satisfactory observations.' 'The idea that the Sun-spots wax and wane in a definite period is too fanciful for acceptance; and still more absurd is the conception that terrestrial magnetism can have any relations whatever with the progress of solar disturbance. We must wait for fresh researches.' 'Who can believe that flames, or clouds, or mountains, many times exceeding the Earth in magnitude, exist upon or close by the Sun? These things must needs be illusions; at any rate, fresh observations are required before such marvels can be admitted.' And as this has happened with facts now accepted, so it is happening, and so (it is feared) it will always happen, as respects many other facts which have been in truth demonstrated, but the demonstration of which does not chance to lie exactly on the surface.

that ten globes such as our Earth might be piled one upon the other on the Sun's surface without attaining to the summit of the highest prominences. But this great fact was not to take its place in our treatises of astronomy until, although twice proved already, it had been proved once again at least.

Accordingly, in 1860, when a total eclipse was to be visible in Spain, preparations were made for finally resolving the problem of the prominences. A host of skilful observers devoted their powers to demonstrating what had already been abundantly demonstrated. It happened fortunately, however, that amongst the astronomers who took part in observing this important eclipse, there were some few who duly recognized the importance of the occasion, and who, therefore, leaving fruitless labours to others, applied themselves to solving important questions respecting the coloured prominences. Their results I now propose to describe; but, in the first place, I will quote the account which Goldschmidt, one of the most skilful telescopists of modern times, gave of the prominences visible on this occasion. Some of the facts recorded by him are of extreme interest and importance, especially as respects the colour of the prominences, since M. Goldschmidt's practice as a painter gave him exceptional experience in this respect.

M. Goldschmidt employed a telescope of four inches in aperture, magnifying about forty times. About *half a minute before totality* he could distinguish 'little grey clouds, isolated in part, and floating outside the

solar disc at some distance from the edges. One of these isolated clouds of a rounded form, and another of an elongated form which touched the exterior edge of the Sun, were observed to be of a grey colour on the ground of the sky, which was a little brighter. An instant afterwards the pyramidical cloud became more clear, and then rose-coloured.' 'I had thus been present,' adds Goldschmidt, 'at the formation of a protuberance'—a remark which has been somewhat misunderstood through being quoted apart from the context. Clearly, Goldschmidt did not mean that under his eyes a prominence had started into existence; but that he had been able to recognise the gradual process by which the prominence became visible with the diminishing sunlight. 'Several smaller prominences,' he proceeds, 'were seen in the neighbourhood of this one, resembling globules of mother-of-pearl, but of an irregular form. These likewise became of a rose colour immediately afterwards, but quickly disappeared. The most imposing as well as complicated of the prominences—which I will call the chandelier (fig. 64) was grand beyond description. It rose up from the limb, appearing like slender tongues of fire, and of a rose colour; its edges were purple and transparent, allowing the interior of the prominence to be seen; in fact, I could see distinctly that this prominence was hollow. Shortly before the end of the totality, I saw escape from the extremities of these rose-coloured and transparent sheaves of light, a slight display in the shape of a fan, which gave to the protuberance a real resemblance to

a *chandelier.* Its base, which at the commencement of the totality was noticed very decidedly on the black limb of the Moon, became slightly less attached, and the whole took an appearance more ethereal and vapourish; however, I did not lose sight of it for an instant. The jets of light which came from the extremities disappeared with the appearance of the first rays of the Sun; but it was not so with the protuberance itself, for, an instant before the end of the totality, I saw several small prominences appear lying

FIG. 64.

close to each other on the right of its base, and forming a square, which is the character of toothed prominences; two others of the same height were seen on the left side of its base when the Sun had already appeared. The north horn of the solar crescent touched the last of these prominences *four minutes and forty seconds after the reappearance of the Sun.* The intense light caused me to abandon this interesting observation, for I was not at the time using a coloured glass; however, I am certain that the "chandelier" and the little prominences at its base had not disappeared up to that moment. Although I am convinced that the protuberances belong to the Sun, nevertheless I

ought to remark that at the last moment I was surprised to see the direction of the *chandelier* referred to the centre of the Moon rather than to the centre of the Sun' (in other words, the chandelier was somewhat inclined). 'The height of the prominence was estimated as about three minutes and a half at the commencement of the totality, and four minutes at the end. A second protuberance (fig. 65) appeared on the apparent right of this, at a distance of about thirty-five degrees,

Fig. 65.

being about $3\frac{1}{3}$ minutes in height and nearly of the form of the symbol of the planet Saturn (♄); this prominence I have called the *hook*. A third, to the right of the two preceding and at a distance equal to that of the two others, assumed a form of which it is difficult to give an idea; however, I will call it the *tooth*. About eleven degrees to the right of the second protuberance, I noticed a fourth, small and in the form of a square; between this and the third there was situated a rose-coloured cloud, the shape of which was elongated and bent, inclined at an angle of forty-five degrees towards the left limb of the Moon. This cloud was entirely detached, floating on the corona like a red cloud at sunset. Its centre was elevated above the

limb of the Moon about one-half the altitude of the other prominences, or about two minutes. A fifth protuberance also appeared at the beginning in the south-east, and was of increased size in the middle of the totality. *I ought to remark that all the protuberances which I noticed had a tendency in their form to describe a curve, the concavity of which was turned from the side of the west.*'

But the principal interest of the eclipse of 1860 attaches to the photographic work of Mr. De La Rue and Fr. Secchi.

To secure a photograph of the Sun itself is a problem of a wholly different character from that which these two astronomers had set themselves; for owing to the intense brilliancy of the solar light, the exposure necessary to secure a photograph of the Sun is of the briefest. For a minute fraction of a second the image of the Sun must fall on the prepared plate; but for no longer interval, or a mere blurred patch would reward the photographer. But to secure a picture of the solar prominences, an exposure of appreciable length is required. Nor was it one of the least difficulties of De La Rue and Secchi that the length of time actually required was unknown to them. All their work was tentative; and there was every reason to fear that success was impossible on account of the colour of the prominences. A red or orange light has commonly no actinic power whatever; insomuch that the 'dark room' of the photographer is, in a photographic sense, nearly as dark when its walls are of orange-coloured glass as

though they were absolutely opaque. So that if the light of the prominences really were pure red, it was hopeless to endeavour to obtain photographs of these objects.*

Despite the difficulties which the problem presented, and the disheartening anticipations which their experience as photographers justified them in forming, the two physicists I have named boldly entered on their task. They adopted different methods. Mr. De La Rue employed the Kew heliograph, and the small image formed at the focus of the object-glass was enlarged before being received upon the plate. Secchi preferred to receive on the plate the image formed by the object-glass of his telescope. This image was about an inch in diameter.

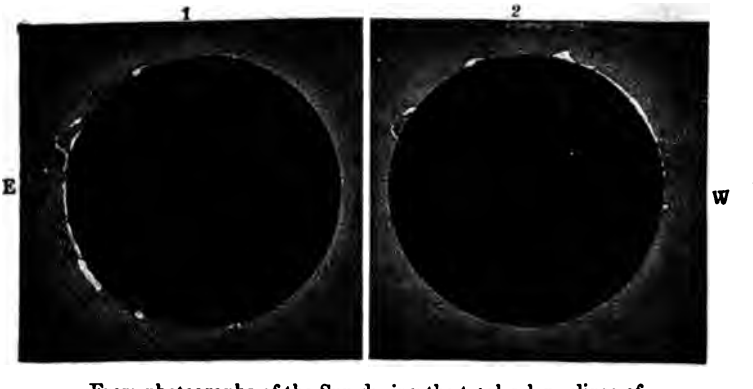
'The result,' says Secchi, 'proved that both systems are excellent, each having its special advantages. In the enlarged image one can distinguish more details, but the direct image gives a greater extension to the corona.'

The two observers were situated at different stations:

* It is worthy of notice, and affords a fresh proof (if proof were wanted) of the fact that observations may involve important results altogether apart from their direct significance, that the successful photographing of the prominences afforded all but complete proof of that which was afterwards demonstrated by the spectroscope—the fact, namely, that the prominences consist of glowing vapour. Secchi or De La Rue might quite confidently have asserted that when the prominences came to be examined with the spectroscope, their spectra would show a band or bands near the blue end of the spectrum, separated by wide dark gaps from certain bands in the red and orange part. The greater part of the light of the prominences corresponds to these latter bands, but that part by which the photographs were taken corresponds to the former.

—Mr. De Le Rue at Riva Bellosa, near the Atlantic; Fr. Secchi at the Desierto de las Palmas, near the Mediterranean. Thus an interval of about six minutes elapsed before the Moon's shadow passed from one station to the other, and an opportunity was afforded of determining whether the prominences change rapidly in figure. Besides this, there was a slight difference in the apparent course traversed by the Moon's disc

FIG. 66. FIG. 67.



From photographs of the Sun during the total solar eclipse of June, 1860. (*De La Rue.*)

in crossing the Sun's; for Secchi and De La Rue were at different distances—and, as a matter of fact, on opposite sides of the path of the centre of the Moon's shadow, so that De La Rue's series of photographs shows more of the prominences on the superior part of the Sun's limb, while Secchi's series shows more of the prominences on the inferior portion.

It will be unnecessary—so closely do the two series resemble each other in all essential respects—to exhibit

both; but further on there will be found a copy of one of Secchi's pictures (fig. 82, p. 324), which the reader may compare with figs. 66 and 67, copied from Mr. De La Rue's photographs.

It will be noticed that fig. 66 represents the earliest phase. The Moon is advancing from right to left, and has just hidden the last fine thread of direct sunlight on the left. Thus we see the full height of the prominences on the left, while no prominences are seen at all on the right of the Sun. At the upper and lower part of the Sun's limb the prominences are partly concealed, and necessarily remain so throughout the eclipse. In fig. 67 the Moon has obliterated a large proportion of the prominences on the left, while it has in turn revealed a number of small prominences, a long range, or sierra, and a lofty and massive projection, on the upper right-hand quadrant. Fr. Secchi observed the prominences directly, and with great care, while his assistants managed the photography. Amongst the phenomena he noticed, I may mention the circumstance that the strange prominence seen in both figures on the upper left-hand quadrant, possessed a helicoidal* structure. In the magnified picture from Mr. De La Rue's photographs, this structure can be clearly recognised.†

* It would be well if this word 'helicoidal' were always employed in this sense, in preference to the word 'spiral,' which might be conveniently restricted to curves lying in a plane. I am not insisting on either term as more correct, but only on the convenience of a recognised term to express spiral curves not lying in one plane.

† Secchi notices the apparent encroachment of this prominence upon the Moon's limb—a peculiarity which he ascribes to the fact that the

Fr. Secchi thus summed up the result of his observations:—

1. The prominences are not mere optical illusions; they are real phenomena appertaining to the Sun. Our observations having been made at two places separated a hundred leagues from each other, it is impossible to suppose that shapes so well defined and so exactly identical could be produced by a phenomenon resembling mirage.

2. The prominences are collections of luminous matter of great brilliancy and possessing a remarkable photographic activity. This activity is so great that many of the prominences which are visible in the negatives could not be seen directly, even with powerful instruments, perhaps because they emitted only chemical rays and few or no luminous rays.*

3. There are masses of prominence matter suspended and isolated like clouds in the air. If their form is variable the variations take place so slowly that it is impossible to recognise their effect during an interval of ten minutes.†

Moon was moving away from the prominence while the plate was under exposure. Doubtless, this circumstance produced its effect; but the phenomenon is chiefly due, according to the experimental researches of Dr. Curtis, to a process of chemical encroachment taking place during the development of the plate.

* The spectroscopic observations made during the American eclipse tend to throw doubt on this conclusion, which, however, is in accordance with the observations made by Mr. De La Rue, and also, be it noticed, with laboratory experiments on the spectrum of hydrogen.

† Later observations show that this opinion must be modified, and that though many of the large prominences remain unchanged in figure for a considerable interval, yet others change very rapidly.

4. Besides the prominences, a zone exists of the same material, enveloping the whole of the Sun's globe.* The prominences spring from this envelope; they are masses which raise themselves above the general level, and even at times detach themselves from it. Some among them resemble smoke from chimneys or from the craters of volcanoes, which, when arrived at a certain elevation, yields to a current of air, and extends horizontally.

5. The number of prominences is incalculable. When observing the Sun directly, its globe appeared to be encircled with flames; there were so many that it seemed hopeless to attempt to count them.

6. The height of the prominences is very great, especially if we notice that account must be taken of the portion concealed by the Moon. Thus estimated, the largest protuberance visible in 1860 was certainly not less than three minutes in height, which corresponds to about ten times the diameter of the Earth; the others had a height of from one to two minutes.

We may consider that the prominences were finally placed in their true place in the solar scheme by the observations made in 1860. Doubts still continued to be expressed by a few; but the majority were satisfied; and henceforth the coloured prominences were very generally regarded as solar appendages.

It may be well to consider briefly the interest of this

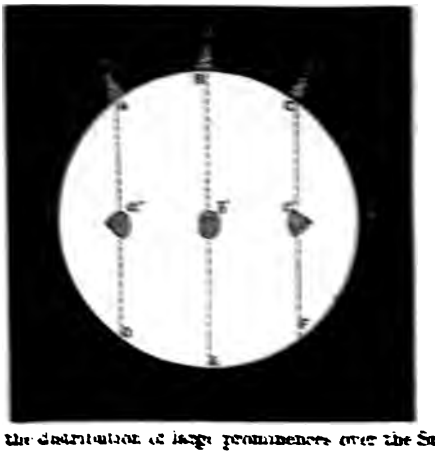
* Grant, Swan, and Von Littrow had already recognised this; and Leverrier, from observations made during the same eclipse, had come to the same conclusion. The matter is alluded to further on. (See note in p. 294.)

result before proceeding to inquire into the researches of the last few years, which even surpass in importance those already described.

In the first place, it must be remembered that though the prominences are seen all round the circumference of the solar disc they do not really form a circle. They are the foreshortened projections of objects which may lie—and many of which *must* lie—thousands of miles from that circle on the Sun which at the moment forms the apparent boundary of his disc. We know, in fact, that certain prominences are as high as three minutes—that is, extend to some 80,000 miles from the Sun's surface. Now, supposing A B C to represent a part of the Sun's circumference, and *a b c* three prominences of this height, an observer, viewing the Sun from a point at a great distance away towards the right or left, would only see the extreme tips of the prominences *a* and *c*, while he would see the full height of the prominence *b*. But in order that these two prominences should be thus in appearance sunk below the solar limb, the line *a c* would need to be about 500,000 miles in length. So that if there were any prominence of so great a height as *a*, *b*, or *c* along any part of the arc A B C it would appear to rise above the Sun's level. Nor need we modify this conclusion on account of the layer of red matter which has been shown by the observations already described to surround the solar globe, since undoubtedly the highest prominences extend considerably more than three minutes (even) above the upper surface of this red layer.

But now, having considered a side view, let us conceive the case of a view from above. Suppose *a′*, *b′*, and *c′* (fig. 68) to represent the same three prominences brought round so as to be viewed from above. Then the actual boundary of the Sun thus viewed would be represented by the circle S B S′, such that whereas *a′ c′* represents 500,000 miles, S S′ represents 850,000 miles. In order, therefore, that a prominence of the

FIG. 68.



Illustrating the distribution of large prominences over the Sun's surface.

full height of 80,000 miles seen from a distant point lying towards the right or left may be visible above the level of the red envelope, it is obvious that it must lie somewhere on the part A D F C, so far as the hemisphere shown in fig. 68 is concerned, or on a similar portion of the other hemisphere. Now, the curved surface A F bears to the whole surface of the hemisphere S D S′ C the same proportion that *a′ c′* bears

to s s′. Hence the extent of surface on which a prominence 80,000 miles high must be placed in order that it should be visible from a distant point is about fifty eighty-fifths, or ten-seventeenths of the whole surface of the Sun. Hence it would follow that if ten such prominences could at any time be counted by the observer, then only some seventeen exist in all probability at that time over the whole surface of the Sun.

Furthermore, it is easily calculated in the same way that for a prominence really as high as those which seem three minutes high to appear so high as two minutes, it must lie on a zone of the Sun including nearly one-third part of his whole surface;* insomuch that if even so many as ten prominences of this height could at any

* The mode of calculation is exceedingly simple. Thus, adding twice three minutes to the Sun's diameter we get about thirty-eight minutes (which may be regarded as including the red layer *as well as* three minutes for the prominences). Now, for a prominence really three minutes high to appear at least two minutes high (I use minutes here as a convenient expression both for real and apparent height) its summit must obviously lie not further than $\sqrt{1 \times 37}$ minutes from a great circle of an imaginary sphere thirty-eight minutes in diameter, this great circle being that whose plane is perpendicular to the line of sight from the observer. Thus the whole zone on which its summit may lie will constitute rather more than twelve thirty-eighths of the surface of that imaginary sphere, and obviously the whole zone on which its base may lie will constitute the same proportion—that is, nearly one-third—of the surface of the Sun itself.

The general rule would be as follows:—The zone on which a prominence—which if really on the limb would appear m minutes high—must lie, in order that it may not appear less than m minutes high, covers a space of

$$\frac{\sqrt{(m-m')\,(d+m+m')}}{d+2m}\,S$$

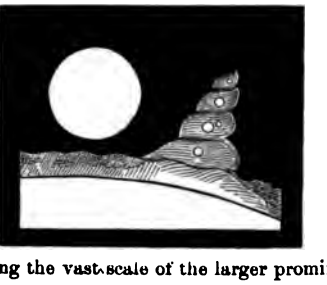
where d is the Sun's apparent diameter in minutes and S the Sun's surface.

time be counted (which cannot but be regarded as a very exceptional circumstance) then only about thirty such prominences exist in all probability at that time over the whole of the Sun's surface. In such a case, also, the total number of prominences actually visible above the level of the red layer should (by the result of the preceding paragraph) be ten-seventeenths of thirty, or about eighteen,—unless, besides the thirty prominences of the height mentioned, there were other inferior ones, in which case more than eighteen would, of course, be visible.

It follows obviously from the comparatively small number of very high prominences ordinarily seen either during eclipses or by the methods presently to be described, that even if the astronomer could roam at his will around the Sun in search of lofty prominences, instead of being limited, as he is in reality, to a single view of the Sun as a disc, he would seldom (if ever) be able to discover many of these amazing objects. Although, taking eclipse with eclipse, the prominences cannot be regarded as exceptional, since every total eclipse yet observed has revealed one or more prominences of an amazing altitude, yet as regards the Sun's surface they are few and far between. So few, indeed, must they be, as to render it extremely probable that not one of those yet seen has been so exactly on the real limb of the Sun as to be seen in its full proportions; nor would it be very hazardous to assign five or six minutes as a height to which the highest prominences, when most favourably seen, may be expected

to attain.* So that we may regard 130,000 miles as no exaggerated estimate of the height of these wonderful objects. If fig. 69 represents a flame four minutes in height, then the four interior (or so-called terrestrial) planets would be represented on the same scale by the four small discs shown on the convolutions—Mercury uppermost, then Venus, the Earth and Moon, and Mars; while even the giant bulk of Jupiter would bear no greater proportion to the enormous solar pro-

FIG. 69.



Illustrating the vast scale of the larger prominences.

minence than is shown by the dimensions of the largest disc in fig. 69.

Let us proceed, however, to those more recent researches by which the actual constitution of the prominences, and even in some sort the part they play in the economy of the Sun's globe, have been ascertained by astronomers and physicists.

Direct observation had been employed during the eclipses of 1842, 1851, 1855, and 1860. Photographic

* Since this was written I received the account of Respighi's observations. In one instance he has seen a prominence no less than six minutes, or about 160,000 miles in height.

records had been obtained during the last of these eclipses. But a method of research more powerful than telescopic observation or photography was now to be applied to the solar prominences. These objects, whether regarded as self-luminous or as shining by reflecting certain portions of the solar light, were clearly well suited for the application of the spectroscope. That powerful mode of analysis which will detect the elements present, even to the minutest conceivable extent, in coloured terrestrial flames, seemed to promise results of the highest interest and importance whenever it should be applied to these coloured solar prominences.

Therefore, Colonel Tennant (to whom astronomy owes a debt of gratitude on this account) early called the attention of the scientific world to the importance of studying the great eclipse of August 18, 1868, not only with the telescope and by means of photographic appliances, but under the searching powers of the most wonderful method of research yet invented by man.

Accordingly, two well-furnished expeditions set forth from England, supplied, through the liberality of the Indian Government and of the learned societies of this country, with the means of satisfactorily pursuing the important investigations which had been suggested by Messrs. Huggins, De La Rue, and other experienced astronomers and physicists.

In the first place it will be well to consider the photographic and other records of this eclipse.

I have selected the six photographs taken by Colonel

Tennant, at Guntoor, as not only affording the most complete illustration of this particular eclipse, but as supplying the best illustration of eclipse photographs yet obtained. The length of time during which the Indian eclipse lasted gave Major Tennant the means of taking a large number of records, with a longer exposure than was possible during the American eclipse; and a value belongs to one series taken at a given station with the same instrument and by the same process, which cannot be assigned to a larger series combining the labours of different observers. It is on this account that I have preferred to present in Plate III. the photographs of the eclipse of 1868, rather than those obtained during the eclipse of 1869.

The instrument employed by Colonel Tennant was a Browning-With reflector of nine inches clear aperture.

'It was determined,' says Colonel Tennant, 'to adopt the Newtonian form, and to place the sensitive plates at the side of the tube in order to find room, which would be very difficult at the end of the tube; the small mirror thus rendered necessary was made unusually large in order that it might reflect all the rays from a surface of some diameter, and it was hoped that by this means, and by giving a sufficient exposure, a picture of the corona might be secured; but light clouds so decreased the actinic power of the light as to defeat this, though still we have traces of the corona.' Colonel Tennant remarks, also, that although, owing to the lowness of the latitude, an entirely new design was required, while 'from the necessities of its construction

the instrument could not be tried before being sent out,' yet the photographic arrangements were very good and convenient, the instrument very firm and steady, while the force required to direct the telescope was very small.

The weather had been remarkably clear for several days before the eclipse, and a magnificent view of the phenomenon would have been obtained had August 18 resembled its precursors. But unfortunately on the morning of the eclipse the eastern sky was clouded over with cumulo-stratus clouds, which although not sufficiently dense to interfere with vision, were very annoying accompaniments to the processes of solar photography. It had been arranged that during the six minutes of totality six plates should be exposed. Notwithstanding the unfavourable condition of the air, this arrangement was carried out; and it was fortunate, as it turned out, that no longer time was given for the exposure of the successive negatives, since otherwise they would in all probability have been spoiled.

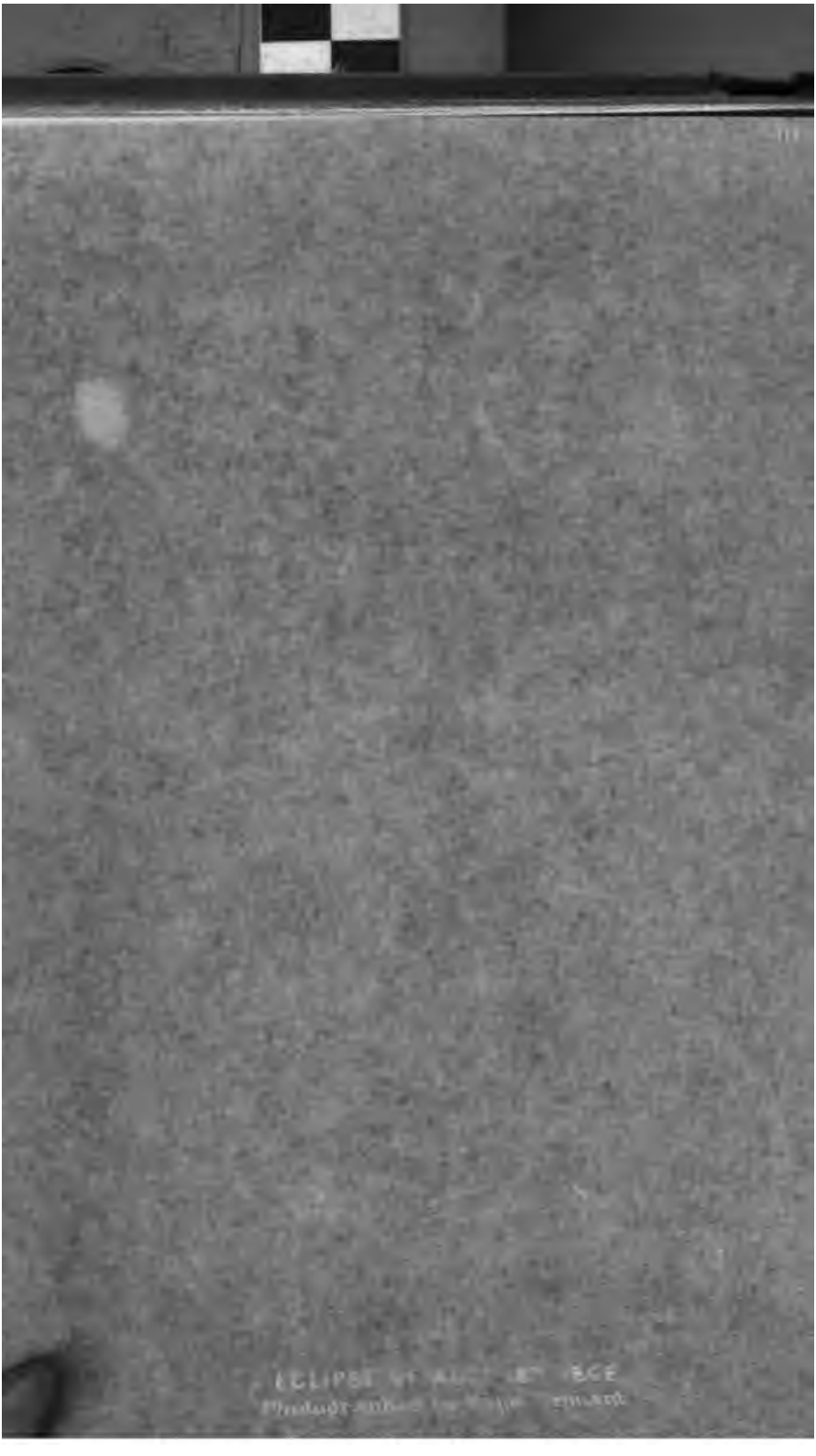
Sergeant Phillips and two Sappers, specially trained for the purpose at Mr. De La Rue's observatory, at Cranford, superintended the subordinate arrangements.

Colonel Tennant thus describes the six photographs (see Plate III.):—

Photograph 1. The exposure was as short as possible—probably less than a second. In this photograph some glare has fogged the plate a little, but the following things are noteworthy:—First. The enormous horn

born [illegible]
seen in Mr. [illegible]
ally double. [illegible]
terminated [illegible]
structure.

Photograph [illegible]
after, and exposed [illegible]
great horn we see a [illegible]
(generally) like a bow, [illegible]
nuclei of light, and having
middle of its base. Next
which has here been over-e
has advanced as far as the
branches seen in No. 1 n
the bifurcation, and the
course larger than in No.
into view. When this ph
[illegible] of the axial struct



on the north side, whose height, as derived from the Moon's diameter (about thirty-four minutes), is three minutes eighteen seconds, corresponding at the distance of the sun to 88,900 miles. Secondly. The ridge of light extending round the upper limb of the Moon, so bright as to be markedly seen through the thickest of the fog, though the glorious horn is paled. Near the horn this light is clearly broken into beads, as may be seen in Mr. James's picture of the horn, and is occasionally double. Thirdly. At its southern end this ridge is terminated by a protuberance of singularly complicated structure.

Photograph 2. Taken almost exactly one minute after, and exposed for five seconds. 'North of the great horn we see a peculiar and faint figure, shaped (generally) like a bow, but with interruptions and nuclei of light, and having an arrow-like spike in the middle of its base. Next we have the great horn, which has here been over-exposed. The Moon's limb has advanced as far as the point where the two lower branches seen in No. 1 meet, leaving only a trace of the bifurcation, and the upper part of the horn is of course larger than in No. 1, as fainter parts have come into view. When this photograph is well illuminated, traces of the spiral structure, so apparent in the rest, are seen; but it would be difficult to recognise it without the other pictures. Passing on, the Moon's limb is on the edge of the faintly luminous stratum I have described in No. 1, which is, of course, now far more marked; and as we approach the south we see

the prominences mentioned under No. 1 as being in the faint stratum, and then reach the upper part of the former terminal prominence, where the long exposure has now blotted out all traces of structure.' *

Photograph 3. Exposed one minute six seconds later, and for ten seconds. 'It is evidently less affected by light than No. 1, and was probably taken through a light cloud. Here the great horn again claims attention; its spiral structure is now clearly made out. The outline of the Moon is wanting till we reach the south terminal protuberance of No. 1, which is now nearly hidden; and in its neighbourhood we again see the flare of which I spoke in describing No. 2.'

Photograph 4. Exposed fifty-two seconds later, and for five seconds. 'Nothing is seen but the great horn, but that picture is of singular beauty. The continually varying intensity of the light and its markedly spiral structure are the material points.'

Photograph 5. Exposed one minute three seconds later, and for a short interval only. 'The great horn is still conspicuous on the east side of the disc; and the outline of the Moon is marked round three-quarters of the circumference. The top of a prominence on the north-west side of the limb is seen.'

Photograph 6. This plate was exposed but for a short time, like the first and fifth, and one and a half

* 'The neighbourhood of both these prominences is marked by light flaring into the corona in irregularly-curved lines,' adds Colonel Tennant, 'and this corresponds to the part of the corona which has been described by Captain Branfill and others as most bright.' This remark will be found important when we are upon the subject of the corona.

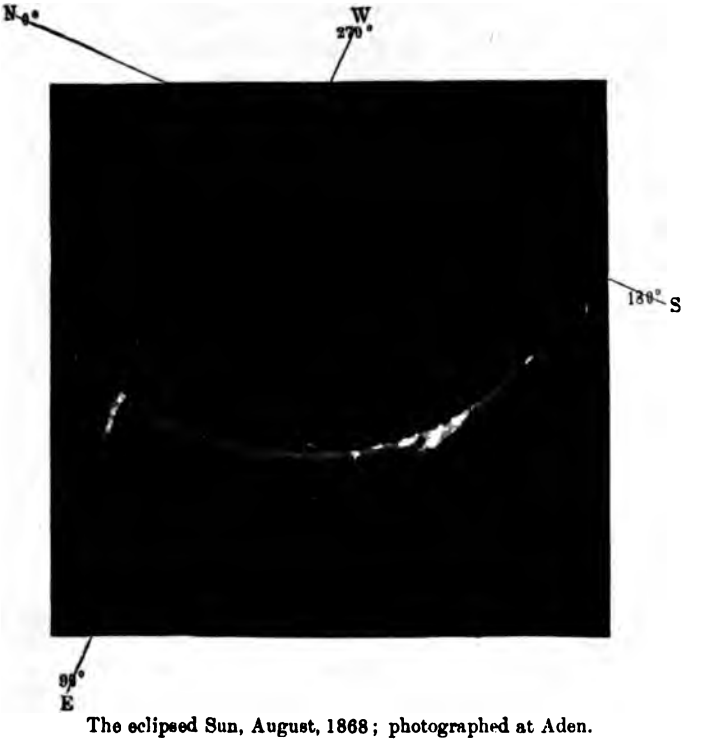
minute after the last. 'It is a picture as late as it was thought prudent to take it during the absence of the Sun. On the south-east side, where the corona was brightest, are traces of it; and the whole limb from north to south-west shows a trace of corona, and a series of beautiful protuberances, only one of which, however, is large. In the original it appears inflated. To me it resembles exactly a toy balloon in the shape of an animal. There is a mark as of an open mouth; the eye and the neck are marked, and the tail stands out stiffly behind. I mention these details because they complete the appearance of inflation by the resemblance they cause to what one has seen in that state. I would especially draw attention to the appearance as of a strong current of air (so to speak) blowing from north to south, and bending over and even detaching and carrying away the tops of prominences.'

It is well worthy of notice, also, that the horn still continues partly visible. It cannot, indeed, have been completely hidden even at the moment when the Sun was about to reappear.*

* I think this account would be incomplete were I not to quote the generous remarks with which Colonel Tennant closes this section of his report. 'Throughout,' he says, 'I have spoken of myself as sharing in the photography. I certainly exposed and developed a few plates, and generally superintended all that went on, and so far I was the superintendent of this department; but the trouble of endeavouring to break in new and unskilled hands, and of carrying out the details, fell on Sergeant Phillips, to whose steadiness and determination to succeed the result in this matter is mainly due. He was, moreover, most useful in other ways, for I could always rely on his care, and was thus enabled to avoid constant exposure to the Sun. Indirectly he has been the means therefore of facilitating my other work.'

At other places along the track of the eclipse similar appearances were perceived; nor have we any distinct evidence pointing to a change of figure during the progress of the Moon's shadow from Aden, in the

Fig. 70.



The eclipsed Sun, August, 1868; photographed at Aden.

west, to Mantawalok Kekee, the most easterly station where the phenomena of the eclipse were witnessed by competent observers. The accompanying picture (fig. 70) represents the eclipse as photographed at Aden

about forty minutes before the shadow reached Guntoor. There is little in this picture to establish the occurrence of a change of figure in the 'great horn' prominence. In comparing it with Colonel Tennant's views it must be remembered (in justice to the German photographers) that the atmospheric conditions were much less favourable at Aden, where the eclipse occurred very shortly after sunrise. In the next chapter a picture (fig. 84) of the eclipsed Sun as seen at the extreme easterly station, Mantawalok Kekee, exhibits a tolerably close general accordance with Colonel Tennant's views. Other pictures and descriptions need not here be referred to, because they differ in no essential respects from those relating to previous eclipses.

Had the work done during this eclipse been limited to what I have hitherto described, the results, however interesting, would not have advanced in any important respects our knowledge of solar physics. We have now, however, to consider far more important results.

At nearly all the stations preparations had been made for observing the coloured prominences and the corona with the spectroscope. All the spectroscopic observers were successful in determining the general character of the prominence spectrum. I select the graphic and lively narrative of Captain Herschel, who commanded the expedition sent out by the Royal Society, and observed the eclipse at Jamkhandi. 'The week preceding the event,' he writes, 'had quite prepared me for disappointment. There seems to be an

annual cloudy and rainy season at Jamkhandi, which lasts about a fortnight, and was said to be somewhat more marked than usual this year. The morning broke, however, as usual—clear, but the driving monsoon-clouds soon showed the kind of sky we were to expect. About a quarter of a minute before totality a third cloud obscured the Sun.' He had directed the spectroscope towards that part of the Sun which would be the last to disappear, and was watching the solar spectrum as it grew gradually narrower and narrower. It will be understood from what has been said towards the close of Chapter III. that the moment this spectrum disappeared the spectrum of the prominence matter (or failing the presence of such matter, the spectrum of the corona) would start into view. 'You may conceive,' he says, addressing Dr. Huggins, 'my state of nervous tension at this moment. Whatever the corona was competent to show must in a few seconds have been revealed; unless, indeed, it should happen that a prominence or *sierra* should be situated at that particular spot, in which case the double spectrum would be presented. But the solar spectrum faded out while it had still appreciable width, and I knew a cloud was the cause.' He turned to the finder of the telescope, removed the dark glass, and waited for the passage of the cloud. As the telescope was mounted so as to follow the motion of the Sun by means of clockwork, there was simply nothing to be done. 'I was,' he writes, 'in that fever of philosophical impatience, which recognises the futility of irri-

tation even while it chafes under the knowledge of fleeting seconds. How long I waited I cannot say—perhaps half a minute. I can well recall the kind of frenzied temptation to turn screws and *look somewhere else*, checked by the calm ticking of the clock telling of a firm hold in the right place, cloud or no cloud. And soon the cloud hurried over, following the Moon's direction, and therefore revealing first the upper edge of the disc, with its radiating, and—as I fancied—scintillating corona; and then the lower. Instantly I marked a prominence (of whose identity there will be no question) near the needle point in the finder. Those few seconds of unveiling were practically all that I saw of the eclipse as a spectacle. With the exception of a hurried glance into the finder at a later period to watch for another break, I was the whole time engaged at the spectroscope. I have not the remotest idea (from actual experience) of the external phenomena which were presented to the thousands of upturned faces of those whose voices I heard outside. I might easily have lifted the curtain and looked out while the clouds were obstructing. That I did not do so is only to be explained by the absence of mind as regarded all else, produced by the concentration of my attention on the problem before me.' Resuming the narrative of his observations, Lieut. Herschel proceeds:—'A rapid turn of the declination-screw covered the prominence with the needle point, and in another instant I was at the spectroscope. A single glance, and the problem was solved. *Three vivid lines—red, orange, and blue!*

I think I was a little excited about this time, for I shouted quite unnecessarily to my recorder, "Red! green! yellow!" quite conscious of the fact that I meant orange and blue.'

The problem was indeed in a general sense solved. Among all the ideas that had been put forward respecting the actual constitution of the prominences one, and one only, now remained available. Had the prominences been self-luminous solar mountains or clouds (properly so called), a continuous spectrum would have been seen; had they been bodies which shine by reflecting solar light the rainbow-tinted streak, crossed by dark lines, which forms the solar spectrum, would have made its appearance. The sole remaining theory was that the prominences consist of glowing gaseous matter.

But it remained now to determine what the nature of this matter might be. To this work Captain Herschel turned his attention, and to this work also all the observers who, like him, had succeeded in solving the general problem, devoted themselves during the remaining minutes of totality. The results they obtained may be thus summed up:—Herschel noted three lines—a red one, which he regarded as possibly C, the red line of hydrogen; an orange one, which he regarded as almost certainly D, the orange (double line) of sodium; and a blue one, which he thought might possibly be F, the blue-green line of hydrogen. Major Tennant saw five lines, which he associated with the lines C, D, *b*, F (probably), and G. M. Janssen saw six—red, yellow, green (two), blue, and violet, and

established the coincidence of the red and blue lines with the lines C and F. M. Rayet saw no less than nine lines, five of which he recognised as brighter than the rest; these he associated with the lines B, D, E, *b*, and F.

But we need not consider the evidence on which these various determinations rest, since a new and far more effective mode of observation was to place beyond all possibility of question the true position of these several lines, and of others as yet undetected.

M. Janssen had been struck during the progress of the eclipse by the exceeding brilliancy of some of the bright lines which formed the prominence spectrum. 'Immédiatement après la totalité,' he writes, 'deux magnifiques protubérances ont apparu: l'une d'elles, de plus de trois minutes de hauteur, brillait d'une splendeur qu'il est difficile d'imaginer.' As he looked, the idea seized him that these lines might be seen when the Sun is not eclipsed. He cried out, he tells us, 'Je reverrai ces lignes-là!'

The principle by which he hoped to effect this will be understood by the reader who has carefully studied the latter portion of Chapter III. Briefly re-stated it is this: The light of the prominences when dispersed by the spectroscope forms a few lines; that of the illuminated terrestrial atmosphere is spread out into the rainbow-tinted solar spectrum. Therefore, if we only use adequate dispersive power, we can cause the prominence-lines to show conspicuously on the background of dispersed atmospheric light.

Clouds which gathered over the face of the Sun soon after totality prevented M. Janssen that day from pursuing this idea. But on the morrow he applied it with complete success. 'I have experienced,' he said, 'to-day a continuous eclipse.'

We have seen how it becomes possible by this method not only to recognise the spectrum of a prominence during full daylight, but by a series of sectional views, so to speak, to determine the figure of a prominence. To this work Janssen applied himself, and he was presently able to forward to Europe news of his complete success in this new branch of research.

The news of this important discovery was nearly two months in reaching Europe, and a few days before it arrived Mr. Lockyer had independently obtained a similar result. The history of his work and of the way in which it was suggested is not without interest. In May, 1866, Dr. Huggins had examined the spectrum of the star which blazed out suddenly in that year in the constellation Corona. He found that this star—now known as T Coronæ*—gave a spectrum which differed altogether from any he had previously examined. There was the rainbow-tinted streak crossed by dark lines which ordinarily forms the spectrum of

* In Mr. Roscoe's valuable work on spectroscopic analysis, this star is always referred to as τ Coronæ. The star τ Coronæ, however, is a well-known fifth-magnitude star towards the north of the constellation. The real variable (ordinarily a tenth-magnitude star) lies to the south of all the conspicuous stars of Corona. It is called T in accordance with the rule by which the variables successively discovered in a constellation are named from the last letters of the alphabet, beginning with R.

a star; but over this, and obviously corresponding to a large proportion of the star's light, there were bright lines. These bright lines corresponded in position with the lines belonging to hydrogen; so that Dr. Huggins was able to pronounce that the great increase in the star's light was due to an outburst of glowing hydrogen. Afterwards he found that other stars, and notably the middle star in the conspicuous W group of Cassiopeia, show bright lines, superposed on the rainbow-tinted background, though relatively far less bright than those seen in the spectrum of T Coronæ. In these cases, also, the lines were those of glowing hydrogen.

Here, then, was a perfectly apt illustration of the principle dwelt on in the concluding part of Chapter III. Here were the lines of a certain element rendered separately visible as bright lines, though the total amount of light received from the glowing hydrogen was not necessarily (in the case at least of the star in Cassiopeia) equivalent, or nearly so, to the remaining portion of the star's light. The concentration of the light into three definite lines enabled it to prevail, as far as intrinsic brightness was concerned, over the diffused light, absolutely greater in amount, which formed the rainbow-tinted streak.

We cannot wonder, therefore, that the idea should at once have been suggested that if any portions of the Sun—as, for example, the coloured prominences—consist of glowing gas, the spectrum of such portions

might be recognisable even amidst the spectrum of the far more intense light (so far as absolute brightness is concerned) of the solar photosphere.

Dr. Huggins and Mr. Lockyer applied their spectroscope unsuccessfully to the search; and Fr. Secchi, who had thought of examining the Sun's edge, gave up the attempt on hearing of Mr. Lockyer's failure to detect anything remarkable there.

News having arrived from India that the spectrum of the prominences included three conspicuous bright lines, it was no longer possible to *discover* the lines by this method. But two months after the eclipse Mr. Lockyer succeeded in seeing them; and the story runs that some five minutes or so before the announcement of Janssen's prior success had been received by the President of the Imperial Academy at Paris, a communication from Mr. Lockyer to a similar effect had been read to that learned body.*

Soon after, Secchi successfully applied the same method, as did Dr. Huggins, whose spectroscope was, however, not altogether well adapted to the work, having been specially designed for stellar researches.

It was a peculiarity of the new mode of observing the prominence-spectrum that it enabled the observer to determine at his leisure, and in a much more satis-

* As regards the question of priority in this matter more importance might be attached to it were it not well known that after Dr. Huggins's observations of T Coronæ, the principle on which the new mode of observation rests was recognised by all who understand spectroscopic analysis. Like the discovery of Sun-spots, therefore, this recognition of the prominence-lines in full daylight was an inevitable sequel of the construction of an adequately dispersive spectroscopic battery.

factory manner than had been possible during the eclipse of 1868, the true position of the prominence-lines, their characteristics as respects shape (the significance of which feature has been already referred to, p. 144), and also the existence of lines which had escaped observation while the eclipse was in progress. It is obvious that the spectrum of the Sun's limb seen at the same time (or that of the illuminated terrestrial atmosphere) affords the most satisfactory means of determining the position of the bright prominence-lines; for *there* in the solar spectrum are those very lines—the dark lines C and D and F, and so on—with which the eclipse observers had associated the bright lines. It is only necessary to see whether those dark lines coincide in position with the bright ones (see figs. 36, 37, and 38, in which, however, but a few dark lines are shown) in order to tell whether the bright prominence-lines are or are not due to the presence of those particular elements to which the solar dark lines belong.

Janssen found in this way that the orange line of the prominences does not correspond, as had been thought,

FIG. 71.

with the D or sodium (double) line of the solar spectrum. He found, however, that the red and blue prominence-lines do exactly coincide with the C and F lines of

hydrogen. So that these enormous objects, extending in some instances to a height of more than 80,000 miles above the Sun's surface, consist in part, at least, of the glowing vapour of hydrogen.

But it was possible to recognise other lines besides these three by the new method, when an instrument of adequate dispersive power was employed. The accompanying picture (fig. 72), for example, exhibits the spectrum of a prominence, and of the adjacent portion of the Sun's limb, as shown by Mr. Lockyer's spectroscope. It will be seen that the double line of sodium as well as three lines of the magnesium spectrum are shown. It is only necessary to suppose that M. Rayet saw the two close double lines as single ones in order to account exactly for the nine lines seen by him.*

* The following is Fr. Secchi's description of the principal lines seen in the prominence-spectrum under ordinary conditions:—

'The line c of hydrogen,' he says, 'is the most easily seen of all. It sometimes reaches the enormous height of three minutes, indicating the presence of such colossal prominences as are seen during eclipses. On the limb of the Sun generally the height of this line is very irregular, and on the average attains to from ten to fifteen seconds. This line also extends in a well-marked manner within the limb, overlapping the disc by ten seconds or more. Yet further on the disc there is a region where the line cannot be seen, being neither bright nor dark, but of the same tint exactly as the neighbouring part of the spectrum, which is thus in this part of its length continuous and uniformly bright. With a slit placed parallel to the limb the space throughout which the line disappears is often considerably extended. Outside the disc the line is much brighter near its base than at its summit, and the line is dilated at the base, and seems to terminate in a point where its light fades off, until, as I have already said, it becomes of the same brightness as the neighbouring part of the spectrum before becoming a dark line. Outside the Sun the line is bounded by two dark lines, which appear at first sight to be the effect of contrast, but may probably have another

Observers were presently able to confirm the views of Grant, Secchi, and others, who had, as already

and a real cause. The bright line is often formed of knots and separate pieces, which are evidently so many fragments of different prominences, placed one beyond the other and unequally bright. If the aperture of the slit be enlarged as much as is possible without rendering the light unbearable, the bright line of the rose-coloured fringe is seen to be marked by irregularities which are due to the roughness of texture (*scabrosità*) of the prominences themselves.'

After mentioning that he has been able to apply Janssen's method of determining the shape of the prominences by taking line-sections, Secchi remarks that he has often tried to determine whether there is any fixed law as regards the direction in which the prominences are bent, but has hitherto not succeeded in tracing any.

'The line in the yellow, near the sodium lines, is about twice and a half as far from the nearest of these as these are from each other. This line is sensibly the prolongation of a bright line in the solar spectrum. In height and brightness it corresponds closely to the c-line, but I have noticed that it will not bear high magnifying power so well. While the F and C lines remain brilliant under such powers, this line becomes weaker, so that only the practised eye can detect it. The line ends in a point, and it often extends itself brilliantly upon the disc of the Sun. Outside the disc it is not bordered by dark lines; on the contrary, with high powers it becomes diffused, and does not stand out sharply like the C-line.

'The F-line is in general not so high as the C-line, and grows faint at the extremity, where it takes the form of a lance. Outside the disc it is accompanied by a narrow dark zone on the more refrangible side. Sometimes I have seen it prolonged beyond the edge of the solar disc, as in the case of the C-line; at other times a very fine black thread shows itself on the more refrangible side. This seems to show that the F-line is not due to hydrogen alone.' (This conclusion, however, has been negatived.)

'The third line of hydrogen near G I have seen as a bright line, but it is necessary to reduce the dispersive power of the prismatic battery in order to obtain sufficient light.'

Secchi is of opinion that where there are faculæ on or close to the limb, there prominences exist; but he is unable to say with equal confidence that wherever prominences exist there are also faculæ.

He was surprised to find on one occasion, when several lines besides the above-mentioned were visible, that of the three lines forming the group *b* of magnesium one and one only was visible,—a second line

mentioned, enunciated the theory that the Sun is wholly surrounded by a layer or envelope of the coloured matter whence as it would seem the prominences spring.* To whatever part of the Sun's edge he directed his spectroscope the bright lines belonging to the prominence-spectrum could be seen, though reaching but to a short distance from the edge of the Sun, save where there is a prominence. Though this layer had been long known, a new name was devised for it, viz., *chromosphere.* The term *sierra* is in better agreement with the now accepted name of the

holding a position midway between the other two lines of the same group. He remarks that Rayet had observed during the eclipse of August, 1868, only two lines of this group, and that these two doubtless corresponded with those seen by himself, of which, as we have seen, one does not accord with either of the two remaining magnesium lines. He was so surprised at this peculiarity that he searched diligently for two hours to detect traces of the other lines of magnesium, but could find none whatever. He considers the objections which have been urged against this observation as 'inconclusive— to use no more severe expression—since it is unreasonable to suggest that he would have been careless when observing so remarkable a phenomenon.'

* Professor Grant wrote, in 1858, 'The zone of a deep red colour observed at Toulon toward the part of the Moon's limb where the Sun was about to emerge clearly indicates the accumulation of nebulous matter in the lower regions of the solar atmosphere, as well as the condensation of the circumambient fluid, of which the latter is composed towards the surface of the Sun, arising from the pressure of the superincumbent strata.' Leverrier wrote in 1860, 'The existence of a bed of rose-coloured matter partially transparent, covering the whole surface of the Sun, is a fact established by the observations made during the time of totality in the eclipse of this year.' Secchi was equally convinced of the existence of this rose-coloured region. 'The observation of eclipses,' he wrote, 'furnishes indisputable evidence that the Sun is really surrounded by a layer of this red matter, of which we commonly see no more than the most elevated points.'

coloured flames; but for some purposes the word *chromatosphere* may be conveniently employed.*

Whether this envelope is to be regarded as a true solar atmosphere, in the sense in which that term is usually understood, may be seriously questioned. As seen in the telescope, it presents a well-defined and very uneven limit, giving the idea rather of a region of flames or clouds than of an envelope of the nature of an atmosphere. That the substance producing the coloured light of the chromatosphere is gaseous admits indeed of no question; but so also the prominences are gaseous. Yet we do not regard these as of the nature of an atmosphere. Now, if the surface of the Sun be covered at all times with small prominences, bearing somewhat the same relation to the gigantic 'horns' and 'boomerangs' seen during eclipses that the bushes covering certain forest regions bear to the trees, then there can be no doubt that the chromatosphere could not rightly be regarded as an atmosphere. We have no evidence that it even extends downwards as far as the visible limits of the photosphere; since undoubtedly if a distance of forty or fifty, nay, even of two or three hundred miles, separated the lower limit of the chromatosphere from the photosphere, no telescopes we possess could suffice (when supplied with suitable spectroscopic appliances) to reveal any trace of this

* It need hardly be remarked that the word 'chromosphere' is incorrectly formed, and can never hold a permanent place in scientific vocabularies. It is as incorrect as *phography* would be for photography, *chromic* for chromatic, or *chronatic* for chronic.

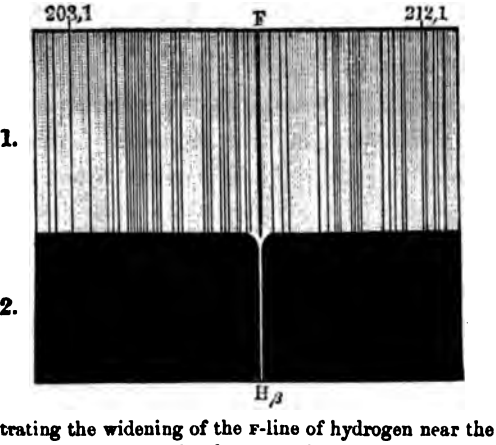
space. A width of two hundred miles at the Sun's distance subtends an arc of less than half a second; and telescopists who know the difficulty of separating a double star whose components lie so close as this will readily understand that a corresponding arc upon the Sun would be altogether unrecognizable.

I am very far, therefore, from accepting with confidence the view that the chromatosphere is the true solar atmosphere. Rather, I believe that it consists of matter, gaseous, no doubt, but suspended in the true solar atmosphere. I think that not only is this view confirmed by the appearance of the chromatosphere as depicted in Plate VI., further on, but that the general resemblance as regards structure and appearance between the sierra and the prominences suffices to render it at least highly probable that the former can no more be described as the solar atmosphere than the latter. But even more striking is the evidence deduced from Plate V. For here we see in the first figure a prominence which has been obviously erupted. We see it as it springs upward, expanding with the diminishing atmospheric pressure. And we see in the second figure how the erupted matter has slowly begun to sink, with expansion, indicating low pressure in the upper regions, but with a figure indicating a real resistance. It is then this invisible atmosphere, through which the upper portion of the prominence has begun to subside, that would seem to constitute the true solar atmosphere, while the sierra should be regarded as consisting of a multitude of smaller prominences,

either freshly erupted or the remains of former eruptions, floating in the lower regions of the solar atmosphere, much as the second prominence of Plate V. is floating (for a brief while) in the upper regions.

The inferences which have been deduced respecting the pressure exerted by the solar atmosphere at and near the level of the photosphere remain unchanged, however, whatever view we adopt. Fig. 72 illustrates the evidence on this point. Here 1 is a part of the spec-

FIG. 72.



Illustrating the widening of the F-line of hydrogen near the base of the chromatosphere.

trum of the Sun's limb, while 2 represents the line F in the spectrum of the chromatosphere. The widening of this line close by the Sun's limb may be regarded as unquestionably indicating an increase of pressure, because the researches of Plücker, Hittorf, Huggins, and Lockyer have shown that the F-line of hydrogen

does actually increase in this way in width when the pressure at which the hydrogen subsists is increased. Temperature, also, has an effect on the hydrogen lines; nor is it quite easy to separate the effects due to pressure from those due to temperature.* But, on the whole, it seems probable that pressure is chiefly in question, while it may be regarded as absolutely certain that temperature alone is insufficient to account for the observed change. Now, whether we regard the glowing hydrogen of the chromatosphere as forming a true constituent of the solar atmosphere or as bearing a somewhat similar relation to that atmosphere that the aqueous vapour in our own air bears to the permanent constituents, it is yet certain that the pressure of the hydrogen near the solar photosphere is a measure of the atmospheric pressure there. And from the observed width of the F-line near the Sun's limb (see fig. 72) it has been estimated by Wüllner that 'the pressure on the base of the chromatosphere, or at the surface of the photosphere, is below the pressure

* In a paper by Dr. Zöllner (*Ueber die Temperatur und physische Beschaffenheit der Sonne*), an abstract of which, by the present writer, appeared in the *English Mechanic* for September, 1870, it is suggested that means might be devised for distinguishing between the effects of pressure and temperature by causing a discharge of gas to take place at the moment when the induction spark is passed through the hydrogen. It is certainly a problem of the utmost importance thus to distinguish between the two effects which play the chief part in solar phenomena.

The disappearance of one of the magnesium lines at a certain height above the spectrum of the limb (see fig. 71) in which the lines B belong to magnesium, is significant of a very low degree of pressure at the level where the shortest line ceases to be visible.

of the Earth's atmosphere.' * He has even assigned the limits of pressure at the level of the solar photosphere as lying certainly 'between 50 and 500 millimetres (or between 2 inches and 20) of a mercurial barometer at the Earth's surface.'

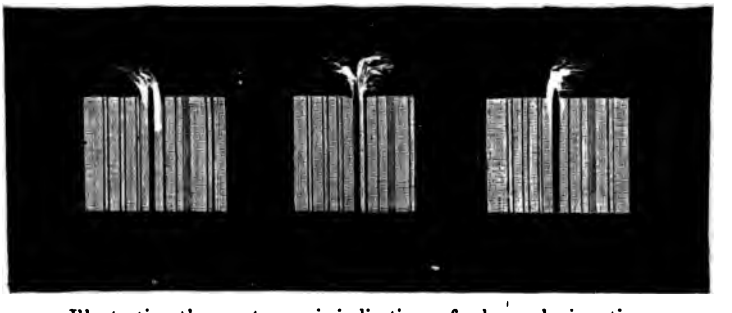
It is within the sierra and certain of the prominences that spectroscopes of high dispersive power exhibit those signs of cyclonic motions taking place in the solar atmosphere, the recognition of which has seemed so surprising and inexplicable. From what has been shown in the last part of Chapter III. it is clear

* In the first edition of this work (1870) I made the following remarks at this stage:—'It must not be forgotten that the width of the hydrogen-line where it actually reaches the spectrum of the limb is not known. The observed width on which Wüllner founded his researches may be that corresponding to a height of 50, 100, or even 200 miles above the photosphere; and within these 50, or 100, or 200 miles an increase of pressure may take place by which the actual density of the atmosphere close by the photosphere may be enormously increased. There may be an atmosphere including the vapours of iron, sodium, magnesium, &c. (of all the elements, in fine, whose dark lines appear in the solar spectrum), extending, say, 100 miles above the photosphere; and yet no instruments we possess could suffice to reveal any trace of its existence, *unless the dark lines in the solar spectrum be thought to demonstrate the fact that such an atmosphere actually does exist.*'

What I thus here suggested as probable was established by an observation made by Prof. Young, of America, during the eclipse of December, 1870. The slit of the spectroscope being so placed as to include the part of the Sun's edge which was last covered by the Moon, included, necessarily, the lowest layers of the solar atmosphere. So long as the Sun's direct light illuminated the spectroscopic field only, a few bright lines belonging to the sierra could be seen. But so soon as the last fine sickle of the Sun's disc was concealed, hundreds of bright lines made their appearance and continued visible for a few seconds,—in fact until the Moon had covered the region thus shown to form a true atmosphere, exceedingly complex, and formed in large part of the glowing vapours of our familiar gases.

that if very rapid motions are taking place, either due to the swift rush of the glowing hydrogen through the solar atmosphere or to the effects of cyclonic motions in the atmosphere itself, by which the glowing hydrogen is borne along, the spectroscope—if only its dispersive power be sufficient—cannot fail to give unmistakable evidence on the point. The problem is altogether simpler and easier indeed than that which Dr. Huggins had to deal with when he undertook to measure the velocity with which Sirius is winging its flight through

FIG. 73.



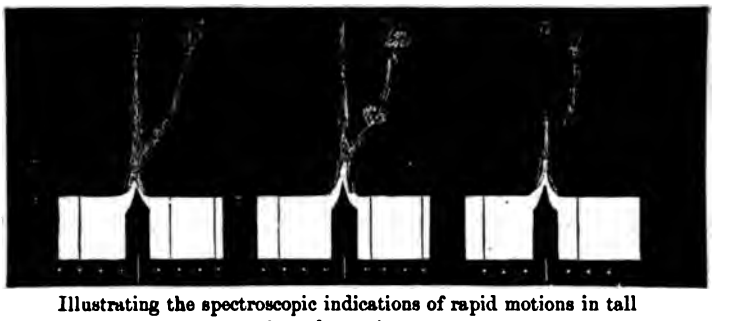
Illustrating the spectroscopic indications of solar cyclonic action.

space. For *there* in the solar spectrum are the very lines of hydrogen with which the chromatospheric lines are to be compared. If the spectroscopic dispersion suffice, the least experienced observer can tell as certainly that a solar storm is in progress as the terrestrial observer can tell by the motions of cirrus clouds that the upper regions of our own atmosphere are disturbed. The accompanying illustration (fig. 73), for instance, shows how the F-line in the chromatosphere-

spectrum is at times swayed (as it were) from coincidence with the dark F-line of the ordinary solar spectrum. At 1 we see the line deflected towards the violet, showing that the portion of the chromatosphere under examination was moving rapidly towards the observer; at 2 we see a deflection both towards the red and towards the violet, indicating that in the same field of view (that is, in the portion of the chromatosphere included within the slit) there were masses moving towards as well as from the eye; while, lastly, at 3 we see a deflection towards the red, indicating a rapid motion from the eye. During some observations such as these, we have had evidence of motions at the almost inconceivable velocity of 120 miles per second. It is impossible, however, to accept Mr. Lockyer's conclusion as to the distribution of the motions over the Sun's surface, because he seems to take very little account of what certainly is the fact, that the extension of the portion of chromatosphere under examination is very great in the direction of the line of sight. Assuming an apparent depth of only ten seconds (which is within the usually observed limits), a tangent-line to the Sun's surface passes through a range of upwards of 60,000 miles; and to speak of the clear recognition of a solar cyclonic storm only 1,500 miles in diameter (as described by Mr. Lockyer) when the visual ray passes through a depth forty times as great, seems to me wholly inadmissible. We have no evidence that the portions of the chromatosphere giving the three displacements shown in fig. 73 may not have

lain 20,000, 30,000, or even 40,000 miles apart, in the direction of the line of view;* in other words, that the observed solar storms, though undoubtedly raging with amazing fierceness, were necessarily cyclonic in character. This remark does not apply with equal force to the evidence deduced from the appearance of the prominence F-line depicted in fig. 74,† because we cannot consider it likely that two prominences of the

FIG. 74.



Illustrating the spectroscopic indications of rapid motions in tall coloured prominences.

enormous height indicated by the extension of the shattered F lines from the spectrum of the limb would

* One may notice in some papers on this subject a mode of explaining the observed phenomena which seems to imply that the spectroscope exhibits to us the conditition of a mere slice of the solar envelopes. The effect of the Sun's solidity in giving an enormous extension to the visual lines through the chromatosphere seems wholly neglected even where a reference to the breadth of the region really included within the slit would appear to invite a reference to the other form of extension.

† In this figure the dots below the spectra indicate the amount of displacement on either side of the normal position of the F-line, corresponding to a velocity of 8, 16, and 28 German geographical miles per second (a German geographical mile being equal in length to a fifteenth part of a degree of the Earth's equator—*i.e.* to about 4⅔ English miles).

lie along the same visual line at the moment of observation.

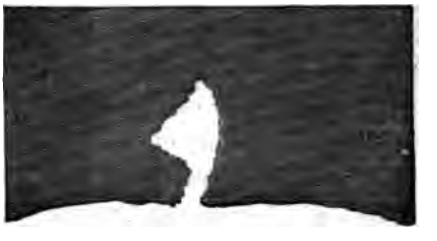
Professor Young, of America, has, however, noted a remarkable circumstance which seems in some sense to throw doubt on the inferences which have been deduced from the displacement of the F-line. He observed on one occasion that while the F-line of hydrogen in a prominence was absolutely *shattered*, the C-line of the same element presented its ordinary appearance. It seems absolutely essential to the interpretation hitherto placed on the displacement of *one* line of hydrogen that the other line should exhibit a similar displacement, differing only to a slight degree as regards extent. If the observation should be confirmed that the C and F lines of hydrogen in the same prominence-spectrum behave differently, a most perplexing problem will be presented not only to astronomers, but to physicists.

We have now, however, to turn to a yet more interesting and valuable series of researches, by which astronomers have been able to observe the exact figure and changes of figure of the prominences. The principle on which these researches, first successfully pursued by Dr. Huggins, have been based, have been already explained towards the close of Chapter III. The visibility of the prominence-lines depends on the dispersive power of the spectroscope; and, clearly, the narrower the opening of the slit the more effective will this dispersion be. The lines of the prominences are thus, indeed, narrowed, but they do not diminish in

brightness; whereas the spectrum of the illuminated atmosphere diminishes in brightness in precise proportion to the narrowness of the slit. But if we widen the slit the forms of the prominences may be seen, if only the brightness of the atmospheric spectrum is not too greatly increased. In order, therefore, that this feat may be achieved, we must have a spectroscope of great dispersive power.

Now, I have said that Dr. Huggins's spectroscope

FIG. 75.



The first prominences seen by means of the spectroscope. (*Huggins.*)

was not particularly well suited for the kind of observation we are considering. It had not, in fact, sufficient dispersive power; so that when the idea occurred to him of seeking for the prominences with an open slit, he had no great hope of succeeding. As a matter of fact, he failed. The increased light blotted out the prominences altogether. But supplementing the powers of his spectroscope by the use of coloured glass, he was able to solve the great problem which was the true end of all the observations hitherto made.

Fig. 75 represents the picture—rough, but instruc-

tive—of the first solar prominence ever *seen* when the Sun was not eclipsed.

So soon as the open-slit method had been suggested by Huggins many other observers adopted it. Mr. Lockyer, availing himself of the great dispersive power

FIG. 76.

A group of solar prominences.—March 14, 1869, 11h. 5m. (*Lockyer.*)

of an instrument Mr. Browning had made for him, found that he could dispense with the use of coloured glasses. The prominences were rendered distinctly visible with the open slit alone; and he could readily watch the changing figures of these mysterious objects. The accompanying drawings (figs. 76 and 77) exhibit two

views of a wild and fantastic group of prominences, drawn by Mr. Lockyer, the second only ten minutes after the first was completed.

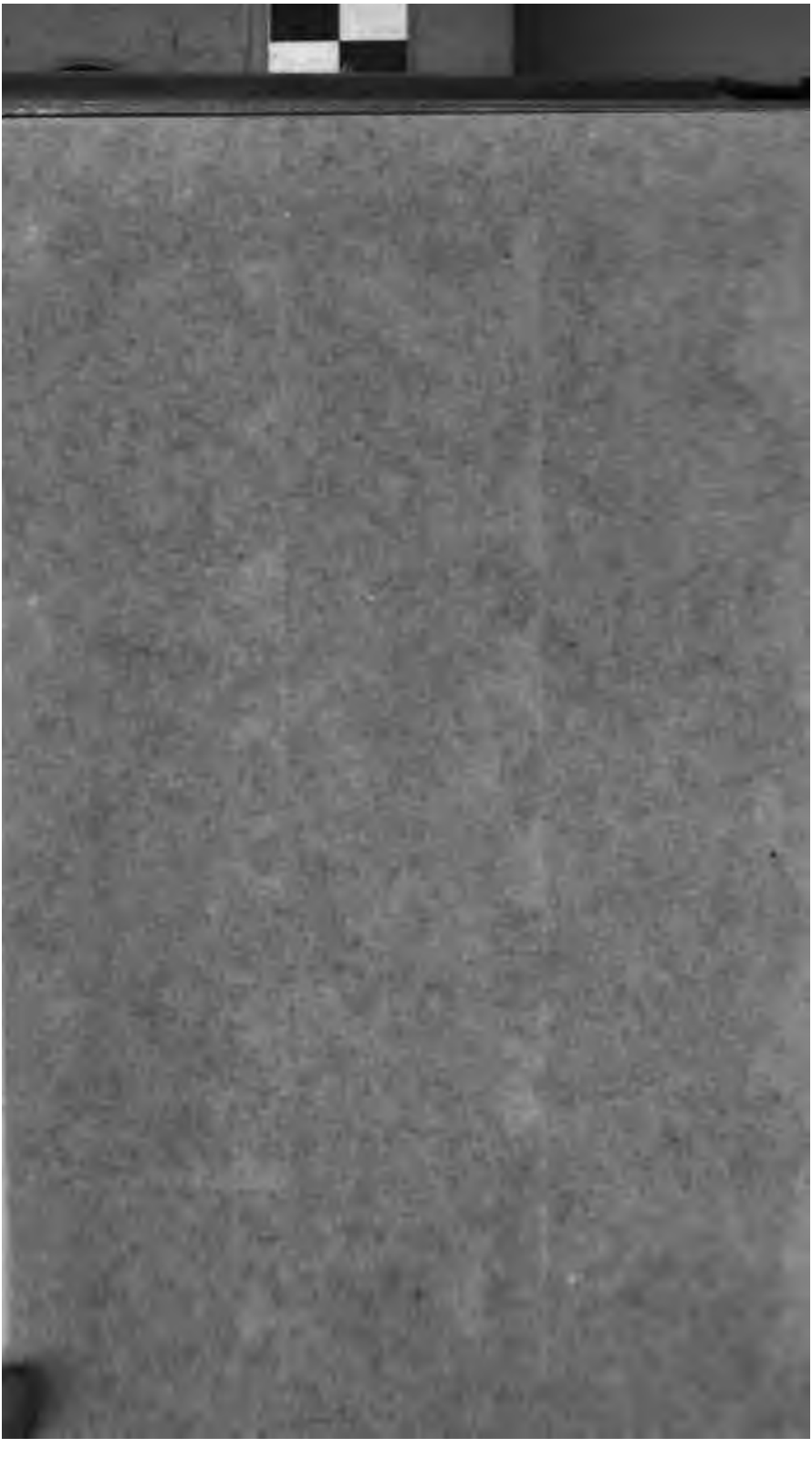
Dr. Zöllner, the eminent German photometrician, applied the same method in a systematic manner.

FIG. 77.

The same group ten minutes later.

Some of the pictures he has published are singularly interesting.

It is to be noticed, in the first place, that Dr. Zöllner observed the same protuberance in three different colours, corresponding to the three lines of its spectrum. He found a material difference between the red and



the blue image on the one hand and the yellow on the other. The latter is very intense only in close proximity to the Sun's limb,* and corresponds there to the other images; but the more delicate details disappear at a greater distance. Zöllner suggests that this may be explained on the hypothesis that 'the rays which give rise to the yellow image emanate from a gas having a greater specific gravity than hydrogen, and therefore existing at a lower level.'

The most interesting of Zöllner's drawings are exhibited in Plates IV. and V. It must be conceived by the reader that these views are only the red images. Zöllner saw perfectly similar blue images, and nearly similar orange-yellow images. The combination of the three produces the complete image as seen, during eclipse, with the telescope.

Respecting the several protuberances shown in Plate IV., he makes the following remarks:—'In fig. 1 we see an intensely luminous peak-shaped mass rising from the border of the Sun, above which is spread a cloud-like formation of less intensity. To the same type belong the protuberances of figs. 4 and 9. In fig. 4 it is remarkable that the surprisingly beautiful cumulus shape of the cloud was separated from the peak by a considerable distance. The cloud was exceedingly delicate, and even its finest details could be recognised.

* It will be observed that Zöllner's results as regards the coloured images correspond with those obtained by Secchi in observing the coloured *lines*.

The separate cumuli of which it was composed appeared almost like dull luminous points.'

Fig. 2 was one of the most remarkable formations. 'I hardly believed my eyes,' says Zöllner, 'when I noticed in it the tongue-like motion of a flame. This motion was slower, however, compared with the size of the flame, than that of high towering flames at great conflagrations. The time required by such a wave in passing from the base to the apex was about two or three seconds. I endeavoured on the following days to verify this observation by the discovery of similar formations, but I have not succeeded, in spite of my arduous and continued search. I therefore request that this statement may be considered as one requiring further proof.'

In figs. 3, 6, and 10 we have remarkable illustrations of the great rapidity with which the protuberances change both in form and brilliancy. In these figures the different shapes are represented which the same protuberance assumed after the intervals of time indicated by the hours and minutes (Leipsic time) named underneath them. The heights range from 30 to 120 seconds, and when it is remembered that a second of arc at the Sun's distance corresponds to a distance of no less than 450 miles, it will be seen on how enormous a scale these prominences are formed.

Zöllner, even at this early stage of his enquiries, was led to arrange the prominences into two distinct classes —the cloud-like and the eruptive. Respecting objects of the former class, he remarks that they remind him

of the different forms of our clouds and fogs. 'The cumulus type is completely developed in some of the figures. Other formations remind one of masses of clouds and fogs floating closely over lowlands and seas, whose upper parts are driven and torn by currents of air, and which present the ever-varying forms so well known, when viewed from the tops of high mountains.' An important observation was made by Zöllner on June 27, 1869. 'On this day,' he says—'the first clear day after a long spell of cloudy weather—I observed the bright protuberance-lines, without, however, being able at that time to make a complete observation of these formations. As soon as I approached the slit of the spectroscope to a certain position in the Sun's limb, where the protuberance-lines appeared particularly long and bright, brilliant linear flashes passed through the whole length of the dull spectrum *over* the limb of the Sun, about three or four minutes distant from the latter. These flashes passed over the whole of the spectrum in the field of view, and became so intense at a certain point of the Sun's limb as to produce the impression of a series of electrical discharges rapidly succeeding one another and passing through the whole of the spectrum in straight lines. Mr. Vogel, who afterwards for a short time took part in these observations, found the same phenomenon at a different portion of the Sun's limb, where protuberance-lines also appeared. 'The phenomenon can be explained,' adds Zöllner, 'by the hypothesis that small, intensely incandescent bodies, moving near the sur-

face of the Sun, emit rays of all degrees of refrangibility, and produce flashes of a thread-like spectrum as their image passes before the slit of the spectroscope.'*

Plate V. illustrates a later paper of Zöllner's, and the most cursory study will serve to show that he observed the prominence, there exhibited, under exceptionally favourable conditions. Nothing can be more striking than the aspect of the prominence shown in these two drawings. It is obvious that Zöllner had here been regarding the active solar eruptive forces casting forth enormous masses of glowing hydrogen gas.† The evidence on this point has already been

* We must not, however, forget the possibility that these objects may have been insects in the air at no very considerable distance from the observer. Capt. Herschel was for some time misled by a similar phenomenon, but, after long watching, one of the bright objects suddenly stopped and flew off in a new direction, and he then recognised the fact that he had been intently observing with the telescope, and carefully analysing with the spectroscope, *a flight of locusts!* These objects were much more conspicuous than those seen by Zöllner, however; and the observation made by Mr. W. S. Gilman, jun., during the American eclipse, that in the heart of a large prominence (a of fig. 78, p. 315) there were exceedingly bright red points, seems to confirm the justice of Zöllner's conclusion.

† Zöllner remarks that the prevalence of one or other of the characteristic forms into which prominences may be divided—viz., the cloud form and the eruptive form—appears to depend partly on their local distribution over the solar surface, partly on the time of observation; insomuch that at a given time one form, at another the other, may be the prevailing type. 'That the cloud-formed prominences present so lively a resemblance to terrestrial cloud and smoke, is easily explained,' he adds, 'when we recollect that the forms of our clouds must be supposed to depend, not on the vesicles of water suspended in them, but (essentially) only on the mode in which the air masses, variously warmed and moved, expand. The water-vesicles in terrestrial clouds form but the material by which such differences of temperature and

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referred to. It seems impossible to conceive that any evidence could be more convincing.

But we owe to Professor Respighi the most systematic and successful attempts yet made to deal with the solar prominences. He has in fact demonstrated the possibility of applying to the prominences systematic observations comparable with those applied by Schwabe and Carrington to the spots. On each of the days, 7, 12–15, 17, 18, 21, 24, 26, 29, and 31 of January, 1869, he took views round the solar limb, until he had completed the circuit, ranging the observed prominences in a straight line (the prominences being presented, for convenience, on a scale twice that of the chromatosphere's horizontal extension). The result is shown in Plate VI. We have only to suppose each horizontal row of coloured prominences bent round a disc representing the Sun, and the prominences reduced to one-half their present dimensions, in order to have a complete picture of the Sun's condition on any of the days illustrated in the plate. It cannot be but that the application of Respighi's system throughout a long series of years will result in revealing laws in the number and magnitude of prominences at different times and in different solar latitudes. Such laws, once clearly recognised, will probably serve to indicate relations, as yet wholly unthought of, between the prominences, the phenomena of the photosphere, the magnetic relations

motion are rendered visible to us. In the clouds of the protuberances this visibility is brought about by the glow of the luminous hydrogen masses.'

of the planetary scheme, and many other periodic phenomena.

Leaving the reader to study at his leisure the singularly interesting features shown in Plate VI., I will briefly indicate certain general conclusions to which Professor Respighi's researches have led him.

He considers that the prominences, at least as regards their origin, bear no analogy whatever to terrestrial clouds, but are strictly phenomena of eruption.* He has noticed that where there are faculæ there are usually prominences, but he considers that the prominences are certainly not identical with the faculæ. Over Sun-spots there are low jets, but no high prominences. In the circumpolar regions great prominences are never recognised, and the prominences actually seen, besides being small, are few in number, and last but a short time. At the solar equator, also, the prominences are less frequent, less active, and less developed than in higher solar latitudes. He noticed some prominences exceeding three minutes, or ten terrestrial diameters, in altitude; and one prominence observed by him had an elevation of no less than twenty terrestrial diameters, or about 160,000 miles. He found that the formation of a prominence is usually preceded by the appearance of a rectilinear jet, either vertical or oblique, and very bright and well defined. This jet rising to a great

* This view is not altogether opposed to Dr. Zöllner's, who regards the two types into which he divides prominences as different forms of the same objects, and, therefore (necessarily), the cloud form as the sequel of the eruptive.

height, is seen to bend back again, falling upon the Sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees more or less rich in branches and foliage. Gradually the whole sinks down upon the Sun, sometimes forming isolated clouds before reaching the solar surface. It is in the upper portions of such prominences that the most remarkable and rapid transformations are witnessed; but a great difference is observed in the rate with which prominences change in figure. The duration, too, is very variable. Some develope and disappear in a few minutes, while others remain visible for many successive days. He considers that the sharply-defined bases of the eruptive jets prove that the eruption takes place through some compact substance forming a species of solar crust.* He agrees with Zöllner in considering that the enormous velocity with which these gaseous masses rush through the solar atmosphere implies that the latter is of exceeding tenuity. His conclusion that repulsive forces must balance the solar gravity, is not shared, however, by Zöllner, who prefers to regard the forces at work in producing the prominences as strictly analogous to those which produce eruptive action on our Earth.†

* This corresponds to the *Trennungsschicht* of Zöllner's theory, who, however, does not agree with Respighi in regarding some form of electric action as the probable cause of these eruptions.

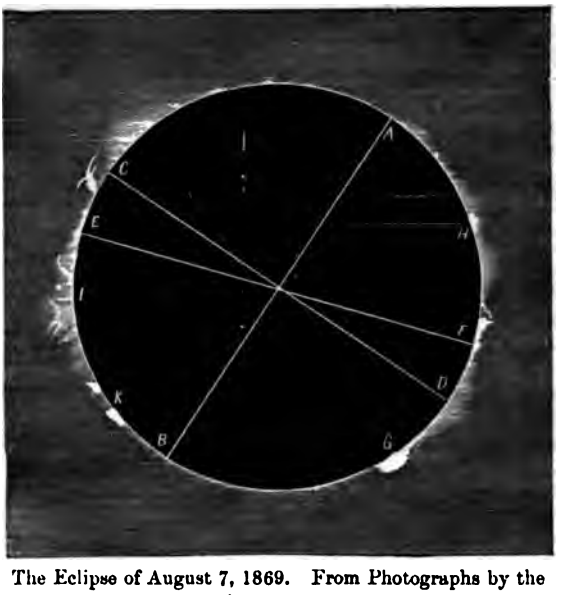
† It seems conceivable that the phenomena of geysers may have their counterpart in these solar prominences. It is true that a gaseous, not a liquid, mass is erupted; so that there is here nothing comparable to the rapid change of a column of liquid into vapour which causes the outbursts of geysers. But the temperature and pressure at which disso-

A few pages further on later evidence on this point will be considered. During the total solar eclipse of August, 1869, many interesting observations were made. But so much space has already been occupied by the subject of this chapter that I must deal very briefly with the observations on the prominences and sierra. In the next chapter I shall have occasion to quote at some length those narratives of the American observers which relate to the corona. The accompanying drawing (fig. 78) exhibits the prominences photographed by the American astronomers, the Moon's disc being reduced so as to permit all the prominences to be visible at once. The line F E indicates the course of the Moon's centre across the Sun, A B being a declination-circle, and C D a declination-parallel. The most successful photographing party was that headed by Dr. Mayer, at Burlington. But the two photographs by Dr. Curtis possess a special value on account of their delicacy and the number of details they exhibit. He has shown that the sharply-defined outlines of prominences, and even of the coronal glare, seen in most photographs of eclipses, result from excessive development. He proves satisfactorily, also, that the encroachment of the prominence-bases on the black disc of the Moon is due to a similar cause.

ciation occurs may bear the same relation to the outbursts of these gaseous matters that the temperature and pressure at which water boils, bear, in Bunsen's theory of geysers, to the occurrence of geyser eruptions. [Prof. Young's account of the explosion he witnessed on September 7, 1871, p. 320, seems to accord well with the view here expressed.]

In Dr. Curtis's contribution to Rear-Admiral Sand's elaborate report of the eclipse the curious reader will find a very interesting investigation of the aspect of the prominence shown between E and C in fig. 78. He refers all its peculiarities of appearance to the action of

Fig. 78.



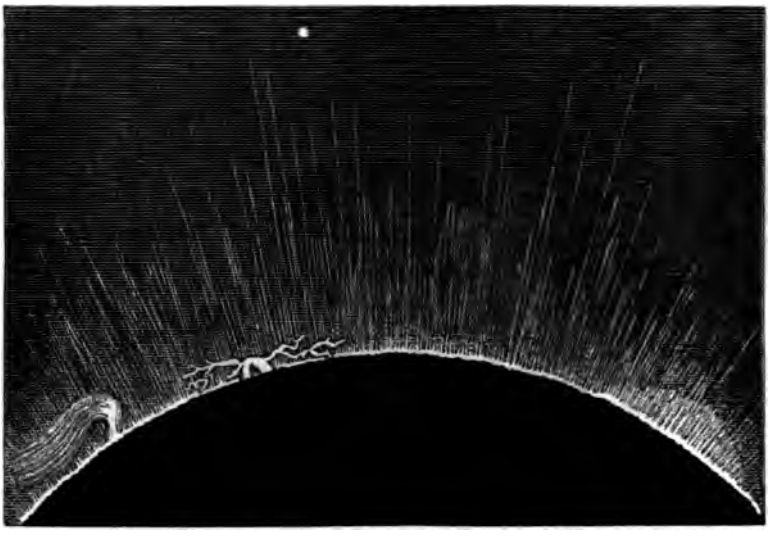
The Eclipse of August 7, 1869. From Photographs by the American astronomers.

a cyclonic storm in the upper regions of the solar atmosphere at this place.

The following account by Mr. G. H. Knight, who studied the prominences with a telescope magnifying 120 times, is extracted from the 'Scientific American' of September 11. But fig. 79 is copied from part of

a very fine drawing, kindly sent to me by Mr. Knight.* The part of the Sun's edge included in the figure is that lying between G and H in fig. 78. 'We have only

FIG. 79.



two precious minutes,' writes Mr. Knight in his narrative of the eclipse, 'and leaving our new acquaintances

* Respecting this drawing Mr. Knight makes the following remarks:—'I believe my rude attempt more true to life than any photographs I have seen. One marked peculiarity of such appearances—the delicate blended haze of light—which to some observers extends much farther into space than to others, is ignored by the photograph, which is an inveterate Rembrandt in its rejection of middle tints. Then, again, the protuberances and the quivering fringe of fire, of which they are the more salient portions merely, are of various tints of gold, copper, and pink, confessedly difficult colours for actinic action, but which the wonderful camera of the human eye takes in perfectly. Besides which, I had the advantage of a more powerful instrument than any other observer I have heard of.'

—Mercury, the sombre woods, the leaden sky, the inky river—to other observers, we direct our 120-magnifier to the red specks, some six or seven in number, now plainly discernible around the Moon's margin.

'These appearances, when brought within the field of the telescope, show a surprising individuality, and all, by shape, suggest violent disturbance, whose motions are, however, of course invisible by reason of the immense distance, and can be ascertained, if at all, only by a record of impressions of successive observers stationed along the track of the swiftly-gliding shadow.

'The tube is directed to a point, near the Moon's nadir (uppermost or inverted in the instrument), occupied by the brightest of these lights (G of fig. 78). The apparition seems to radiate from some point hidden behind the Moon's disc, beyond which it emerges in brilliant silver, copper, and ruby-coloured coruscations, the copper tints predominating, and terminates in a circular arc like a half-set Sun. The impression conveyed to an observer is of a vast explosion from a centre some twenty thousand miles below the edge of the Sun's disc, and extending therefrom about fifty thousand miles in every direction.

'About fifty degrees of the Moon's circumference from this prominence we observe a second and wholly different one (F of fig. 78) which bears a grotesque resemblance to a stag's antlers or to the strands of a raveled rope tossed about by a whirlwind. The shape and coruscations of this apparition suggest electrical

action (fancy an electric spark 500 miles thick!) or the deflagration of some liquid metal. Its colour is crimson; its height about twenty thousand miles.

'Still another and totally different emanation is seen (H of fig. 78). It wears the semblance of a horse's tail, or, more nearly, of a puff of smoke drifting northward, and illuminated by the rosy hues of sunset.

'At this stage of observation some one jogged the instrument, and before it could be adjusted to another group, a glint of sunlight from the disc's right margin blinded our unaccustomed retinas and flooded the landscape with returning day. At the same instant, looking upward, we beheld the Moon's black shadow, *sharply defined as a wall in the air*, sweeping majestically away from right to left before our very eyes —and the total eclipse of 1869 had become a thing of the past!'*

The spectroscopic study of the coloured prominences, since 1870, has revealed some facts of extreme interest.

In the first place, Fr. Secchi has made a series of observations by which the researches of Zöllner and Respighi are confirmed and supplemented. The following are the chief points in Secchi's latest papers which it seems necessary to notice in this place. I have

* 'With our present meagre array of facts,' notes Mr. Knight, 'hypotheses are premature. The Sun's heat seems too intense for many of the terrestrial phenomena of chemical action. A cause may, however, exist in meteorolites which, falling with inconceivable velocity and possessing a high spheroidal repulsion, may carry with them into the Sun's seething cauldron a comparatively cold body of disturbing elements and give rise to the mechanical and other perturbations whose manifestations have been noted.'

italicised the facts to which I desire to invite special attention.*

He notes that the chromatosphere presents four aspects,—(1) smooth, with a defined outline; (2) smooth, with no definite outline; (3) fringed with filaments; and (4) irregularly fringed with small flames.

He divides the prominences into three orders—heaps, jets, and plumes. The heaped prominences need no special description here; they will be recognised in Zöllner and Respighi's views. The jets are the prominences to which Resphighi's description at p. 312 is specially applicable. Secchi notes that their *luminosity is intense*, insomuch that they can be seen through the light clouds into which the sierra breaks up. *Their spectrum indicates the presence of many elements besides hydrogen.* When they have reached a certain height they cease to grow, and become transformed into exceedingly bright masses, which eventually separate into fiery clouds. The jet prominences last but a short time, rarely an hour, *frequently but a few minutes*, and *they are only to be seen in the neighbourhood of the spots. Wherever there are jet prominences there also there are faculæ. The plume prominences* are distinguished from the jets in not being characterised by any signs of an eruptive origin. They often extend to an enormous height, last larger than the jets, though

* Fr. Secchi's paper was received with enthusiasm by the Academy of Sciences, at Paris, and the whole memoir, though exceeding the usual space, was printed *in extenso* in the *Comptes Rendus*.

subject to rapid changes of figure, and, lastly, *they are distributed indifferently over the Sun's surface.* '*It would seem,*' says Secchi, 'that in jets *a part of the photosphere is lifted up*, whereas in the case of plumes *only the chromatosphere is disturbed.*'

On September 7, 1871, Prof. Young, of America, observed the most remarkable outburst from the Sun ever yet witnessed by man. His observation is singularly interesting in itself; but it acquires a fresh interest from the light it throws on observations already described, and more particularly on those made by Respighi and Secchi.

'On the 7th of September last,' he writes in October, 1871, 'between half-past twelve and two P.M. there occurred an outburst of solar energy remarkable for its sudden violence. Just at noon the writer had been examining with the telespectroscope * an enormous protuberance or hydrogen cloud on the eastern limb of the Sun.

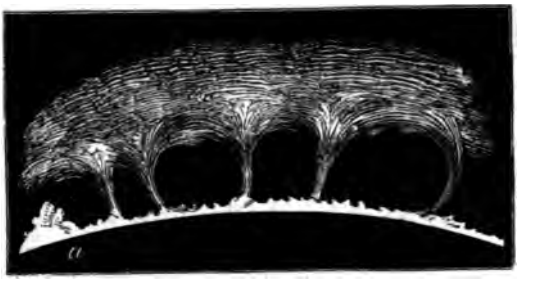
'It had remained with very little change since the preceding noon—a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, and floated above the chromatosphere with its lower surface at a height of some 15,000 miles, but was connected with it, as is usually the case, by three or four vertical columns brighter and more active than the rest. Lockyer compares such masses to a

* 'This is the name given by Schellen to the combination of astronomical telescope and spectroscope.

banyan grove. In length it measured 3′ 45″, and in elevation about 2′ to its upper surface—that is, since at the Sun's distance 1″ equals 450 miles nearly, it was about 100,000 miles long by 54,000 high.

'At 12.30, when I was called away for a few minutes, there was no indication of what was about to happen, except that one of the connecting stems of the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another at the northern end a little brilliant lump

FIG. 80.



had developed itself, shaped much like a summer thunder-head. Fig. 80 represents the prominence at this time, *a* being the little "thunder-head."*

'What was my surprise, then, on returning in less than half an hour (at 12.55), to find that in the meantime the whole thing had been literally blown to shreds by some inconceivable up-rush from beneath. In place of the quiet cloud I had left, the air, if I may use the

* 'The sketches do not pretend to accuracy of detail, except the fourth; the three *rolls* in that are nearly exact.

expression, was filled with flying *débris*—a mass of detached vertical fusiform filaments, each from 10″ to 30″ long, by 2″ or 3″ wide, brighter and closer together where the pillars had formerly stood, and rapidly ascending.

'When I first looked some of them had already reached a height of nearly 4′ (100,000 miles), and while I watched them they rose with a motion almost perceptible to the eye, until in ten minutes (1.5) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurement; the mean of three closely-accordant determinations gave 7′ 49″ as the extreme altitude attained, and I am particular in the statement because, so far as I know, chromatospheric matter (*red hydrogen* in this case) has never before been observed at an altitude exceeding 5′. The velocity of ascent also, 166 miles per second, is considerably greater than anything hitherto recorded. A general idea of its appearance when the filaments attained their greatest elevation may be obtained from fig. 81.

'As the filaments rose they gradually faded away like a dissolving cloud, and at 1.15 only a few filmy wisps, with some brighter streamers low down near the chromatosphere, remained to mark the place.

'But in the meanwhile the little "thunder-head" before alluded to had grown and developed wonderfully into a mass of rolling and ever-changing flame, to speak according to appearances. First it was crowded down, as it were, along the solar surface; later it rose

almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads which were most curiously rolled backwards and downwards, like the volutes of an Ionic capital; and finally it faded away, and by 2.30 had vanished like the other.

Fig. 81.

Figs. 82 and 83 show it in its full development; the former having been sketched at 1.40, and the latter at 1.55.

'The whole phenomenon suggested most forcibly the idea of an explosion under the great prominence, acting mainly upwards, but also in all directions out-

wards, and then after an interval followed by a corresponding in-rush; and it seems far from impossible that the mysterious coronal streamers, if they turn out

FIG. 82.

to be truly solar, as now seems likely, may find their origin and explanation in such events.

'The same afternoon a portion of the chromatosphere

FIG. 83.

on the opposite (western) limb of the Sun was for several hours in a state of unusual brilliance and excitement, and showed in the spectrum more than 120 bright lines whose position was determined and cata-

logued,—all that I had ever seen before, and some 15 or 20 besides.'*

It remains only to mention that Professor Young, of America, has succeeded in obtaining a photograph of a prominence when the Sun has been shining in full splendour. Although the result is, he tells us, not remarkable as a presentation of a solar prominence, yet as indicating the possibility of applying photography to record the condition of the chromatosphere and prominences from day to day, from month to month, and from year to year, it is full of promise. The time seems not far off when we shall be as familiar with the laws according to which these mysterious objects appear, develope, and disappear within the solar atmospheric envelope as we have already become with the general laws affecting the behaviour of Sun-spots.

* *Boston Journal of Chemistry.*—Prof. Young has published a list of 103 of these chromatospheric bright lines, whereof 5 have been detected by Rayet, 16 by Lockyer, 3 by Lockyer and Janssen, 5 by Lockyer and Rayet, and the remaining 74 (besides the 17 above referred to) by Young. The elements whose existence has been recognised in the chromatosphere, are hydrogen, iron, sodium, magnesium, barium, calcium, chromium (?), nickel, and titanium, the presence of the last being a discovery of Prof. Young's.

CHAPTER VI.

THE CORONA AND ZODIACAL LIGHT.

THE coloured prominences, as we have seen, are phenomena which have been recognized only in recent times. We have now to deal with a phenomenon which has been known to astronomers for a much longer period, but has proved more difficult of interpretation, and remains even at the present day not clearly understood.

It may fairly be believed that during the earliest total solar eclipses observed by mankind, the corona, or crown of glory, which surrounds the black disc of the Moon, must have attracted attention. Yet the records of the recognition of this phenomenon are by no means so distinct as might have been expected. We do not find, indeed, in ancient works professedly treating of the phenomena of nature, any reference to the imposing appearance presented by the solar corona. The earliest allusion to the phenomenon is held by Professor Grant to be that passage in the Life of 'Apollonius' where Philostratus, speaking of the signs and wonders which occurred before the death of Domitian, says, 'In the heavens there appeared a prodigy of this nature:

a certain corona, resembling the Iris, surrounded the orb of the Sun, and obscured his light.' Taken alone, this passage would certainly not seem intended to describe the phenomena of a total eclipse; but as Philostratus afterwards remarks that the darkness was so great as to resemble night, we may assume with some confidence that a total solar eclipse had occurred.

Plutarch more distinctly describes the appearance actually presented by the corona when he endeavours to explain why the darkness during a total eclipse is not so great as that of night. 'Even though the Moon,' he says, 'should hide at any time the whole of the Sun, still the eclipse is deficient in duration as well as amplitude, for a peculiar effulgence is seen around the circumference which does not allow a deep and very intense shadow.'

I do not propose to record in full the observations which have been made upon the corona. To do so would occupy, indeed, much more space than can here be spared. Referring the reader who wishes for a more complete account of the earlier observations to Professor Grant's admirable 'History of the Physical Sciences,' I shall consider here those observations alone which tend to throw light on the nature of the corona.

Some of the earlier observers of total solar eclipses would seem to have been misled by the great brightness of the corona close by the Sun, and to have supposed that a ring of direct sunlight had remained uncovered.

We shall see presently that modern observers have also been struck with the brightness of the light close by the Sun; and it seems obvious that this bright light is to be regarded as wholly distinct from the light of the chromatosphere, the redness of which is too marked to escape recognition. Remembering that before the invention of the telescope the corona was the only marked phenomenon to which observers were able to direct their attention, a certain weight attaches to their comments on the brightness near the Moon's disc.

Clavius having expressed his belief that the eclipse of 1567 was annular, Kepler was led to investigate the subject, and he proved that that eclipse must needs have been total. In 1605 he witnessed a total eclipse at Naples, and found, in the features it presented, the explanation of the remarks of Clavius. 'The whole body of the Sun,' he says, 'was completely covered for a short time, but around it there shone a brilliant light.' We might suppose that he referred to the chromatosphere, because he says that the light was 'of a reddish hue;' but as he adds that it was 'of uniform breadth, and occupied a considerable part of the heavens,' there can be no doubt that he is speaking of the corona.

Dr. Wyberd gives a remarkable account of the appearance of the corona during the total eclipse of March 29, 1652. 'When the Sun was reduced to a narrow crescent of light,' he says, 'the Moon all at once threw herself within the margin of the solar

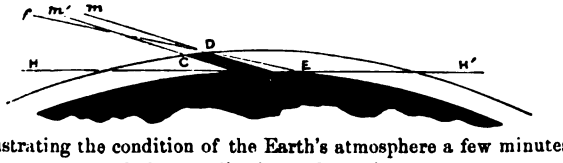
disc with such agility that she seemed to revolve like an upper millstone, affording a pleasant spectacle of rotary motion. In reality, however, the Sun was totally eclipsed, and the appearance was due to a corona of light round the Moon, arising from some unknown cause. It had a uniform breadth of half a digit, or a third of a digit at least;* it emitted a bright and radiating light, and appeared concentric with the Sun and Moon' when the middle of totality was reached.

It need scarcely be remarked that the agility with which the Moon seemed to throw herself within the margin of the Sun's disc was merely apparent. It has been noticed by many observers that the total obscuration of the Sun seems to occur suddenly, the Moon covering the last sickle of sunlight apparently at a leap. Irradiation is no doubt in question. The sickle of light, even when in reality it is indefinitely fine, appears, through the effects of irradiation, to have a definite breadth; so that the Moon seems to traverse a definite distance in obliterating what is in truth the finest possible curve of light. But beyond this there is a circumstance which cannot but give an appearance of somewhat agile motion to the eclipsing disc of the Moon. Up to the very moment when totality is about to begin, the air between the observer and the Moon is illuminated by direct sunlight. This is easily seen by a consideration of fig. 84, in

* A digit signifies the twelfth part of the solar diameter. The term is nearly obsolete.

which A B C D represents the portion of the Moon's shadow in the atmosphere, E the place of an observer towards whom this shadow is swiftly advancing. Then, until the shadow actually reaches E (at which moment totality begins) a line drawn from E to D will pass to the left of the line A D *m*—as in the direction E D *f*; so that, since the line A D *m* is necessarily directed towards the Moon's limb, the directly illuminated air (bounded, of course, by A D) extends between the observer and the Moon. Hence, the Moon's disc seems lighted up by this atmospheric glare until the

FIG. 84.



Illustrating the condition of the Earth's atmosphere a few minutes before totality in a solar eclipse.

very moment when totality begins; and as the curtain of glare is drawn suddenly away towards the edge where the last sickle of the Sun's direct light is vanishing, all the circumstances tend to give an appearance of agile motion in that direction.

As respects the rotatory motion which seemed to accompany this leap forward on the Moon's part, we can very well understand it as referring to the completion of the corona, which must needs take place by a sweeping round of the bounding rays to close in upon each other opposite the point where the last part of the Sun's disc disappears. It will be well to notice,

however, as we proceed, whether we have convincing evidence of an apparent motion of the coronal beams after totality has begun. The evidence on this point cannot but have an important bearing on the views we are to form respecting the corona.

Dr. Wyberd saw a corona of very limited extent—indeed only half a digit wide—corresponding to a height not half so great as that of many prominences which have been observed during recent eclipses.

Our next observation refers, however, to a much more favourable view of the corona.

In May 1706 Plantade and Capiés saw a bright ring of white light surrounding the eclipsed Sun, and extending to a distance equal to about one-tenth part of the Moon's apparent diameter. Outside this bright ring a fainter light could be recognised, which extended no less than four degrees from the eclipsed Sun, fading off insensibly, until its light was lost in the obscure background of the sky.

In 1724 Maraldi noted a circumstance of some importance. At the beginning of the total eclipse which was observed in France in that year he perceived that the corona was wider on the side towards which the Moon was advancing than on the opposite side. At the close of totality the widest part of the corona was on the opposite side. As this would exactly correspond to what would be observed if the corona lies beyond the Moon, and so is traversed by the Moon precisely as the Sun himself is, it will be well for us to notice as we proceed whether Maraldi's observation

has been confirmed or disproved, or how the evidence stands with respect to it.

In 1733 a total eclipse occurred in Sweden, which was observed in a manner reflecting great credit on the astronomers of that country. The Royal Society of Sweden invited all who could spare the time to assist, as far as their ability permitted, in recording the phenomena presented during total obscuration. Accordingly there are few eclipses—perhaps there is not one—to the phenomena of which so many independent witnesses give testimony. 'At Catherinesholm the pastor of Forshem noticed that a ring of light which appeared round the disc of the Moon was of a reddish colour—an observation confirmed by Vallerius, another pastor, who noticed, however, that at a considerable distance from the Sun the ring appeared of a greenish hue. The pastor of Smoland states that "during the total obscuration the edge of the Moon's disc resembled gilded brass, and that the faint ring around it emitted rays in an upward as well as in a downward direction, similar to those seen beneath the Sun when a shower of rain is impending." The mathematical lecturer in the Academy of Charlesstadt, M. Edstrom, observed these rays with special attention, and remarks, respecting them, that "they plainly maintained the same position, until they vanished along with the ring upon the reappearance of the Sun." On the other hand, the ring, as seen at Lincopia, seemed to have no rays. Professor Grant remarks that "from the descriptions given by several observers, it would seem that at the

commencement of the total obscuration the ring appeared brighter and broader at the part of the Moon's limb where the Sun had disappeared, but that towards the close of the obscuration it was more conspicuous in both these respects at the part where the Sun was about to emerge."'*

It is observable, therefore, of this well-watched eclipse that it confirms Maraldi's observation, while the variety of appearance presented by the corona at different stations would point to the conclusion that if the corona is not a phenomenon of our own atmosphere, its light must for the most part be of a very delicate nature, insomuch that seeming unimportant differences in the circumstances under which it is viewed suffice to modify its aspect to a very noteworthy extent. We shall see presently that during recent eclipses similar evidence has been afforded of the extreme faintness of a large portion of the coronal light.

During the eclipse of 1766 the corona exhibited four remarkable expansions, separated from each other by nearly equal intervals.

In the account given by Don Antonio d'Ulloa of the appearance presented by the total eclipse of 1778, we again find a reference to the appearance of rotatory motion in the corona. 'Five or six seconds after the commencement of the total obscuration,' he writes, 'a brilliant luminous circle was seen surrounding the Moon, which became vivid as the centre of that body

* From an article in the *Cornhill* for October 1870 (by the present

continued to approach the centre of the Sun. About the middle of the eclipse its breadth was equal to one-sixth of the Moon's diameter. There appeared issuing from it a great number of rays of unequal length, which could be discerned to a distance equal to the lunar diameter. It seemed to be endued with a rapid rotatory motion, which caused it to resemble a fire-work turning around its centre. The colour of the light was not uniform throughout the whole breadth of the ring. Towards the margin of the lunar disc it appeared of a reddish hue; then it changed to a pale yellow, and from the middle to the outer border the yellow gradually became fainter, until at length it seemed almost quite white.'

The next eclipse during which new features of importance were noticed, was that of 1842. We have seen how carefully the prominences were observed during that eclipse, and how many eminent astronomers were engaged in noting the phenomena presented during the total obscuration of the Sun. The corona was also carefully studied. Indeed, one is inclined almost to regard it as a misfortune that the same astronomers who tell us about the appearance of the prominences, are those from whom we derive our information respecting the corona. One cannot but feel that the accurate observation of both phenomena was more than could be expected even from the most skilful astronomers, and that a division of labour might have been advisable.

The apparent motion of the corona was noticed by

several observers. Some of those stationed at Montpellier thought that the corona had a rotatory motion. Francis Baily compared the appearance of the corona to the flickering light of a gas illumination. Otto Struve, also, was much struck by the violent agitation to which, as it seemed to him, the light of the ring was subjected. The apparent extent of the corona as seen by different observers varied in a somewhat surprising manner. Otto Struve, observing at Lipesk, found the breadth of the corona equal to the Moon's apparent diameter; while M. Petit, observing at Montpellier, assigned to the corona a breadth scarcely exceeding one-fourth of this amount. Baily's estimate lay between these values. Nor were the observers in agreement as to the general appearance of the corona. Otto Struve observed several luminous expansions, some of them extending fully four degrees from the Moon's limb. Signor Picozzi, observing at Milan, saw two jets of light occupying a position which corresponded very nearly with that of the ecliptic. Several observers in France noticed a similar peculiarity. Minor rays, also, were distinctly recognised by Mauvais at Perpignan, and by Baily at Pavia. The last-named astronomer remarks, indeed, that the diverging rays were sufficiently marked to deprive the corona of the appearance of a ring. But Mr. Airy, observing the corona from the Superga, could scarcely recognize any radiation whatever; and he remarks, that 'although a slight radiation might have been preceptible, it was not sufficiently intense to affect in

a sensible degree the annular structure by which the luminous appearance was plainly distinguished.'

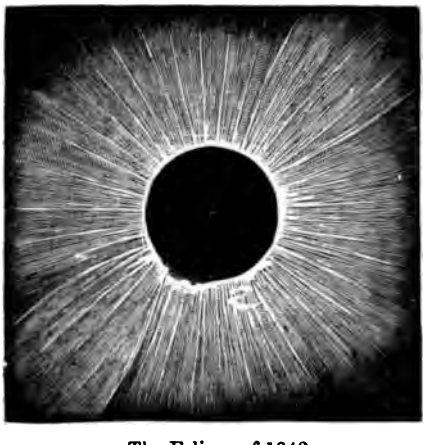
If we consider the accounts which the observers give of the brilliancy of the corona, we shall find that these peculiarities are in accordance with the theory that atmospheric conditions were alone in question. At the Superga the brightness of the corona seemed no greater than that of the Moon; but at Pavia, where Baily was stationed, the splendour of the corona was much greater. 'I had imagined,' this observer writes, 'that the corona, as to its brilliant or luminous appearance, would not be greater than that faint crepuscular light which sometimes takes place in a summer evening, and that it would encircle the Moon like a ring. I was therefore somewhat surprised and astonished at the splendid scene which now so suddenly burst upon my view.' We have seen that at Lipesk the corona seemed to extend much farther from the Sun than elsewhere, and accordingly we find that its brilliancy was also far greater. The light was so bright, Struve writes, that the naked eye could scarcely endure it. Many could not believe, indeed, that the eclipse was total, so strongly did the corona's light resemble direct sunlight.

But perhaps the most interesting and important observations made in 1842 are those which refer to the structure of the coronal light. It was noticed at Montpellier that the light of the corona was not uniform, nor merely marked with radiations, but that, in places, interlacing lines of light could be seen. Arago,

at Perpignan, recognised this peculiarity with the naked eye. He saw, 'a little to the left of a diameter passing through the highest point of the Moon's limb, a luminous spot composed of jets entwined in each other, and in appearance resembling a hank of thread in disorder.'

The accompanying picture (fig. 85) represents the

Fig. 85.



The Eclipse of 1842.

general phenomena seen during the eclipse of 1842; but it must be remembered that such illustrations cannot be regarded as accurately representing details, because they are usually drawn after the eclipse is over, and represent merely what the observer remembers. Until the whole duration of a total eclipse is devoted by a skilful observer and draughtsman to the delinea-

tion of the corona, we cannot expect to have really trustworthy views.

During the eclipse of 1851 no observations were made which tended to throw new light on the nature of the corona. It is worthy of notice, however, that the Astronomer Royal found the aspect of the corona different from what he had noticed in 1842. 'The corona,' he says, 'was far broader than that which I saw in 1842. Roughly speaking, the breadth was little less than the Moon's diameter, but its outline was very irregular. I did not notice any beams projecting from it which deserved notice as much more conspicuous than the others; but the whole was beamy, radiated in structure, and terminated—though very indefinitely—in a way which reminded me of the ornament frequently placed round a mariner's compass. Its colour was white, or resembling that of Venus. I saw no flickering or unsteadiness of light. It was not separated from the Moon by any interval, nor had it any annular structure. It looked like a radiated luminous cloud behind the Moon.'

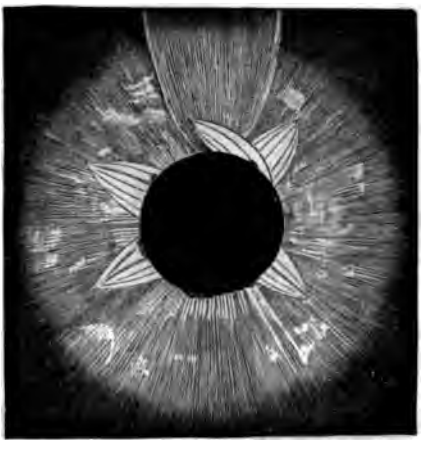
These observations, combined with what had been before noticed, seemed to demonstrate that the aspect of the corona is variable according to the circumstances under which it is viewed. It does not seem to be established that the rotatory and flickering motions suspected by other observers were only optical illusions, though the observation of the steadiness of the corona, by such an observer as the Astronomer Royal, goes far to negative observations of motion by less experi-

enced astronomers. In the case of a phenomenon like the corona, it is easier to imagine movement in the ring of light than to become convinced of its fixedness.

The eclipse of 1858, visible in Brazil, is chiefly remarkable on account of the strange drawing made by the French astronomer Liais (fig. 86). During the late eclipse (Dec. 1872), the accuracy of this singular picture was confirmed.

The eclipse of 1860 was remarkable as the first in

FIG. 86.



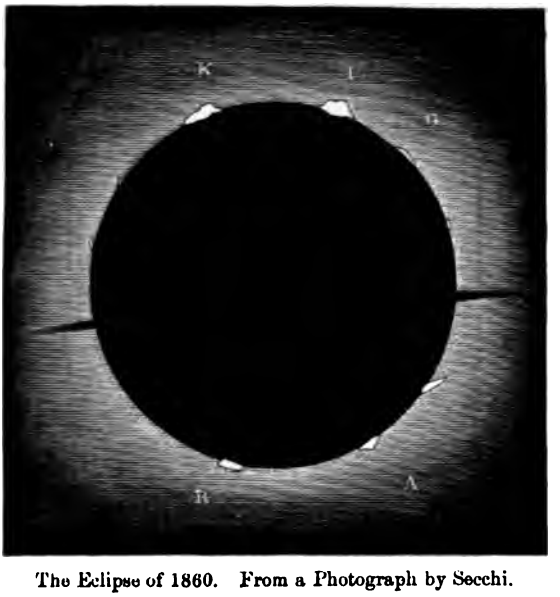
The Eclipse of 1858. (*Liais.*)

which the powers of photography were employed to aid in the resolution of the problems presented by the corona. It will be seen, on a reference to figs. 66 and 67, that Mr. De La Rue succeeded in obtaining traces of the corona. Those seen in Father Secchi's photographs are somewhat more distinct, the method he

employed giving a smaller and more fully illuminated image. In a third of Fr. Secchi's photographs (fig. 87) the corona is yet more distinctly shown.

The extension of the corona in fig. 87 is regarded by Fr. Secchi as corresponding to the solar equator, whose position is very nearly indicated by the cross-wire

FIG. 87.



The Eclipse of 1860. From a Photograph by Secchi.

shown in the figure. 'La couronne,' he says, 'est très irrégulière, mais on peut remarquer qu'elle présente une étendue plus considérable à droite et à gauche que dans les autres directions, c'est-à-dire qu'elle est plus développée dans le plan de l'équateur (solaire) que suivant la ligne des pôles.' But, as

Fr. Secchi himself points out further on, the figure indicates rather an extension opposite four points lying between the equator and poles, than an extension at the equator. In fact, fig. 87 presents a very striking resemblance to Mr. Whipple's picture further on (fig. 90). It is worthy of notice, too, that not only does the outline of the corona present this quadrilateral aspect, but in the bright parts close to the Moon's limb there are four corresponding regions of greatest brilliancy. In the second of Mr. De La Rue's photographs the brightest portions seem similarly disposed.*

Some of the direct observations made in 1860 serve also to throw important light on the nature of the corona. To the Astronomer Royal the corona presented much the same aspect as in 1851. Bruhns of Leipsic states that when the last rays of the Sun disappeared, the corona shone out with a white light

* One cannot wholly agree with Father Secchi's remark, that Mr. De La Rue's photographs afford no evidence of the peculiar quadrilateral expansion of the corona corresponding to the position of the zone of spots. It is true, however, that when Fr. Secchi 'published his results astronomers did not conceal their doubts.' If the expansion of the corona in four directions be regarded as an ordinary phenomenon (and we have seen how often it has been noticed), some very perplexing questions would be presented as to the cause of the peculiarity. A rectangular figure, like that shown in fig. 87, would correspond to a cylindrical real figure; but it would also correspond to other figures of three dimensions. It is indeed possible that there may be no corona at all opposite the solar poles, the light we see there being merely a foreshortened view (on this supposition) of the great extension over the spot belts. In this case the true figure of the corona would resemble that due to the rotation of two hyperbolas, having the same axes, around one of these axes.

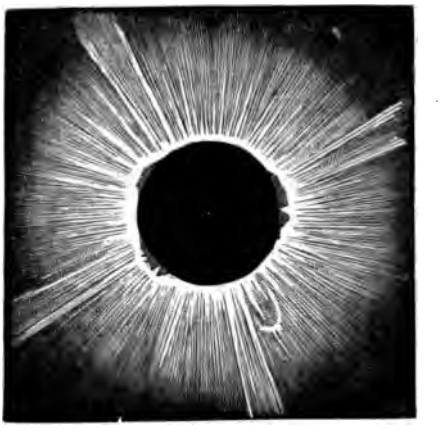
of such brilliancy that the protuberances were almost obliterated. He adds, 'the black-looking Moon was surrounded by a clear light of unequal breadth. It was considerably wider below than above, varying from nearly one-half to a quarter of a degree, and its general appearance gave me the idea that the Moon was eccentrically placed within it. Its general outline was circular, but on the eastern side a long ray shone out to a distance of about a degree (that is, twice the Moon's apparent diameter); it was of a tapering figure. During the ten seconds that my attention was directed to it, neither the direction nor the length of the ray varied; its light was considerably feebler than that of the corona, which was of a glowing white, and seemed to coruscate or twinkle. My assistant, M. Auerbach, noticed, in the south-western part of the corona, a curved ray about a tenth of a degree in length.' Father Secchi found that the corona could be seen with the naked eye for about forty seconds after the reappearance of the Sun, 'the solar light shining like an electric lamp projecting tremulous shadows.' Mr. De La Rue states that 'several minutes before totality the whole contour of the Moon could be distinctly seen: when totality had commenced, the Moon's disc appeared of a deep brown in the centre of the corona, which was extremely bright near the Moon's limb, and appeared of a silvery white, softening off with a very irregular outline, and sending forth some long streams. It extended generally to about from seven to eight tenths of the Moon's diameter.' This description corresponds

very satisfactorily with the appearance represented in the accompanying picture (fig. 88) by Feilitsch.

During the eclipse of April 1865 it was noticed that the corona continued visible for thirty-six seconds after the appearance of the first rays of direct sunlight.

The eclipse of March 1867 was only annular, yet it presented a feature well worth careful consideration,

Fig. 88.



The Corona during the Eclipse of 1860. (*Feilitsch.*)

and that at a station considerably removed from the line of central eclipse. O. Struve and Wagner, using the great equatorial of the Pulkowa observatory, noticed that when the eclipse reached its greatest phase, the outline of that part of the Moon's disc which was outside the Sun's, could be distinctly seen. Schwabe, who observed the same phenomenon at Dessau, noticed that this part of the Moon's disc was

rendered visible by its superior blackness; a circumstance which proves that the light of the corona must be considerably stronger than the illuminated atmosphere near the Moon's place,* even when a portion of the Sun's direct light is falling upon the air there.

The total eclipse of August 1867 attracted very much less attention than those which occurred during the same month of the two following years. Yet the observations made by Herr Grosch, of the Santiago observatory, Chili, in conjunction with Vice-Director Vergaza and Lieut. Vidal, are full of significance in connection with the main object of this chapter, which is the determination of the true theory of the corona. At the moment when totality began, 'there appeared,' says Herr Grosch,† 'around the Moon a reddish glimmering light, similar to that of the aurora, and almost simultaneously with this (I mean very shortly after it) the corona. This reddish glimmer, which surrounded the Moon with a border of the breadth of at most five minutes, was not sharply bounded in any part, but was extremely diffused, and less distinct in the neighbourhood of the poles. I can decidedly confirm this at least as regards the Sun's north point, but not so much so of the south point, as that part was less observed by me, but was more particularly attended to by Lieutenant Vidal,

* By the atmosphere near the Moon's place, I mean that part of the atmosphere which lies nearly in the same direction as the Moon. Of course it is, in reality, very far removed from the Moon's true place.

† I quote (from the *Student* for March 1869) Mr. Lynn's translation of Herr Grosch's narrative.

who could not afterwards give any more positive information concerning this phenomenon. . . . To speak now of the corona:—its extent was considerably longer in the direction of the Sun's equator; and in considering its nature, we must, I believe, look upon it as decidedly unconnected with him.* Whereas, in the direction of its poles, its apparent height exceeded that of the Moon by only a third of her diameter; in the direction at right angles to this, its extent amounted to four-fifths of that diameter. Its light was white, brighter on the Moon's limb, and becoming gradually fainter on the other side. This white light *was not in the least radiated itself*, but it had the appearance of rays penetrating through it; or rather as if rays ran over it, especially in the direction of east and west, forming symmetrical pencils diverging outwards and passing far beyond the boundary of the white light. These rays had a more bluish appearance, and might best be compared to those produced by a great electromagnetic light. Their similarity to these indeed was so striking that under other circumstances I should have taken them for such, shining at a great distance. The view of the corona here described is that seen with the naked eye. I employed but a very short time upon it, only as much, in fact, as was necessary to obtain a mere momentary view of the general appearance of the totality. And now, in conclusion, I

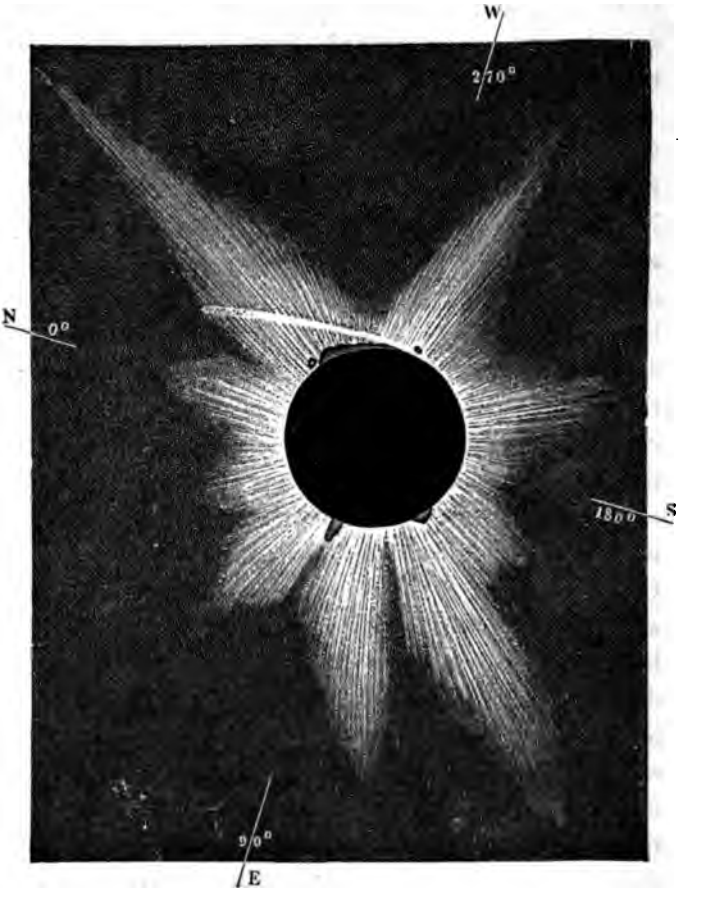
* Herr Grosch obviously does not mean that the corona is not, in his opinion, a solar appendage, but that there is no continuous connection between the Sun and the corona; that it is not, in fact, of the nature of a solar atmosphere.

would just mention another phenomenon, which seemed to me too important to pass unnoticed. It showed itself exactly at the north point of the Sun. In the white light of the corona, close upon the Moon's limb, there appeared several dark curves. They were symmetrically arched towards the east and west, sharply drawn, and resembling in tint lines drawn with a lead pencil upon white paper. They gave the impression as if they proceeded *from one point*, which point was on the other side of the Moon; not, however, on the limb, but somewhat nearer the centre of the Sun. Beginning at the distance of one minute, they could be traced up to about nine minutes from the Moon's limb. Throughout the duration of the eclipse *they underwent no alteration whatever*, remaining constant both in form and colour until the disappearance of the corona. Lieutenant Vidal (agreeing with Signor Vergaza) speaks of a similar appearance, exactly at the south point, in the form of a fan or sheaf-formed tuft of light; but he says nothing of characteristic dark curves, such as I saw at the north point. There is, however, no cause to doubt the accuracy of the observed facts. These dark curves indicate a strong magnetic polar force of the Sun, so that an appearance of the kind in question might be seen at the south as well as at the north point; but perhaps being less developed at the former than at the latter, it was less perceptible.'

It is impossible to read this account without feeling how much might be learned from a systematic survey

of the corona during any considerable total eclipse. Here were three observers only, who each gave to the

FIG. 89.



The Corona during the total Eclipse of August 1868, as seen at Mantawalok-Kekee.

corona but a fugitive attention, and yet their accounts agree in pointing to the existence of appearances which merited a thorough study during the whole continuance of totality, and that not by two or three observers only, but by ten or twelve at least, each appointed to the investigation of some special feature.

The study of the corona was one of the subjects to which the attention of the observers sent out to view the great eclipse of August 1868 was specially directed. More seems to have been hoped from the application of spectroscopic and polariscopic analysis than from direct observation; and, accordingly, the best accounts we have of the general aspect of the corona are those derived from observers not belonging to the expeditionary parties. We owe to the professors of the college of Manilla, who observed the eclipse at Mantawalok, the accompanying very remarkable drawing of the corona. It is important to notice that, unlike most pictures of the corona, this one can be trusted. Owing to the want of success which had attended all attempts to photograph the corona, the professors were led to think of an ingenious plan for obtaining exact drawings. They prepared beforehand several sheets of paper, which were introduced one after another into a dark chamber, so that the image of the eclipsed Sun fell upon them, and the features of the corona were rapidly sketched out on each. The corona is described as of a somewhat triangular aspect; yet it is to be noticed that there are four rays of the longer sort, so that the corona in this case, as in so many others,

exhibits a general approach to the trapezoidal figure. The slightly curved streak of white light crossing the longest of the coronal beams is a very remarkable feature. This streak is described as of an intensely white and uniform light. It appeared (or was at least first noticed) some two minutes after the beginning of the totality, and remained visible until the Sun began to reappear.

The polariscopic observations, made during this eclipse, on the light of the corona were not successful. The observers agreed, indeed, that the light of the corona was polarised in a plane through the Sun's centre; a circumstance which, if confirmed, would go far to prove that the corona shines by reflecting the Sun's light; but the Astronomer Royal, who has carefully examined their accounts, considers that little dependence can be placed on the evidence. I may as well add that, during the eclipse of 1869, the American observers obtained a different result, and that, in the opinion of those best competent to judge, the question of the polarisation of the corona's light in a plane through the Sun's centre remains still in abeyance.

Colonel Tennant examined the light of the corona with the same spectroscope which, as we saw in the last chapter, has given information of such interest respecting the coloured prominences. He saw a faint continuous spectrum. 'Thinking that want of light prevented my seeing the bright lines which I had fully expected to see on the lower strata of the corona, I opened the jaws of the slit.' He still failed to recog-

nise any signs of bright lines. '*What I saw*,' he writes (the italics are his) '*was undoubtedly a continuous spectrum, and I saw no lines.* There may have been dark lines, of course, but with so faint a spectrum, and the jaws of the slit so wide apart, they might escape notice.'*

Before discussing this result, I proceed to mention other evidence bearing on the same point.

During the eclipse of August 1869 several of the American observers renewed the attempt to determine the exact nature of the corona-spectrum. Their results were not accordant. Professor Pickering obtained a faint continuous spectrum crossed by three bright lines. Professor Harkness recognised only one bright line on a continuous background. After observing the spectrum of the prominences, he asked Professor Eastman, who was directing the telescope, to bring the corona into the field. A bright part of the corona was thus brought under examination, but *no spectrum appeared.* 'I asked him to try another place. Still nothing was visible; and, raising my head from the instrument for the first time since the commencement of the totality, I remarked, "Can't see any spectrum; don't believe we will get any." "Oh! yes, we will,"

* It is strange that, notwithstanding the very plain account given by Colonel Tennant, it should continue to be asserted that, according to his observations, the corona gave a solar spectrum; that is, a spectrum crossed by the Fraunhofer lines. The American observers were so misled by this assertion as to search specially for the dark lines which Colonel Tennant was supposed to have seen. The obvious meaning of his narrative is, that he saw a continuous spectrum, without either dark lines or bright lines.

said he. At that instant it struck me that perhaps the slit was too narrow; so I opened it a little, and then again placed my eye at the instrument. In the meantime Eastman had put the needle at a very bright part of the corona, and I at once saw a continuous spectrum, about as bright as that given by the full Moon on a clear night. Remembering that the observers in India, in August 1868, had said that the corona gives a continuous spectrum *with absorption lines*' (a mistaken idea, as mentioned in the last note), 'I looked very carefully for them; but, to my great surprise, I could see none, and I am perfectly satisfied that none were visible in my instrument. On the contrary, I saw an absolutely continuous spectrum crossed by a single bright line, whose position was recorded.' This line was in the green, and, if actually in the place assigned by Harkness, would correspond to a line belonging to the spectrum of copper. But as he makes the line coincident with one of the prominence-lines, it seems certain that it can be no other than a line of iron, close by the E lines, which has been seen by several observers in the spectrum of the prominences.* Professor

* This iron line appears also as a bright line in the spectrum of the aurora, according to the best observations hitherto made. As I write, I receive from Mr. Browning an account of his observations on the aurora of October 25, with one of his miniature spectroscopes. He saw a bright line near E, and another not far from B. Mr. Birmingham, of Tuam, with a similar instrument, saw the usual bright line in the green, one not far from it to the left, very faint, and one of medium brightness near F. Professor Wenlock notes four lines in the yellow-green part of the spectrum, and one somewhat more refrangible than the F-line. All these accounts are reconcilable when we remember the extreme faintness of the auroral light, and the fact that no exact determination by the

Young paid particular attention to the spectroscopic observation of the corona. He also had been misled by erroneous accounts respecting the Indian observations, and so expected to see a faint solar spectrum. He found, on the contrary, that the light of the corona gave a faint-continuous spectrum* crossed by one bright line. He suspected, also, that there were two other lines. He regarded it as certain, from the close agreement of the bright line (and of the two suspected lines) with Professor Winlock's picture of the aurora-spectrum, 'that the corona is simply an electric discharge, no doubt varying with great rapidity, as we see in the case of the aurora; in fact, that the corona is a permanent solar aurora.'

Now, although these accounts seem at first sight discordant, they can be brought into agreement, not only with each other, but with Lieutenant-Colonel Tennant's, by a consideration of the circumstances under which they were severally given. Tennant, seeing only a continuous spectrum, opened the slit somewhat widely: 'the jaws of the slit were wide

method of coincidences has ever yet been attempted. I had written further to the effect that, 'even when the auroral light is only ruddy to the eye, no red lines are seen, so that we may conclude that the excess of red is due to a peculiarity in the light of mixed refrangibility forming the continuous spectrum,' when I learned that, on the evening of October 24, Mr. J. R. Capron, with one of Browning's small direct-vision spectroscopes (adapted to star observation), had succeeded in observing a line in the red, 'very much like the lithium line, but rather more dusky. It was only well seen in the rosy patches of the aurora, but could be faintly traced wherever the rose-tint at all extended.'

* He found also that the light of the faint-continuous spectrum was polarized.

apart,' he says; too wide, I imagine, to show the bright lines. For, from what is shown at p. 143, it will be seen that the brightness of the coronal bands or lines could not be increased in this way, though their *breadth*, and so the total amount of light from them, would be increased in precisely the same proportion as the opening of the slit. But the *brightness* of the continuous background would be increased in this same proportion. Hence the bright lines which Tennant could not see, on account of their fineness, were changed by opening the slit into broad bands of no greater brightness, and rendered invisible by reason of the increased brightness of the background. An intermediate amount of opening would in all probability have shown the lines. Now we see that Professor Harkness failed even to see a continuous spectrum when he used a narrow slit; and the fineness of the lines (not nearly so brilliant as the prominence-lines) caused them to escape his notice precisely as had happened with Tennant. But when he opened the slit 'a little,' he saw the continuous spectrum and one bright line. Professor Young saw the same under similar circumstances. Had the slit been opened somewhat more they would not have seen that bright line, but would have failed as Tennant had, and for the same reason. There is no difficulty in explaining why Professor Pickering saw three bright lines, since he was using an integrating not an analysing spectroscope (see p. 140), and a large part of the light forming the spectrum he observed must

have come from the coloured prominences and the sierra.*

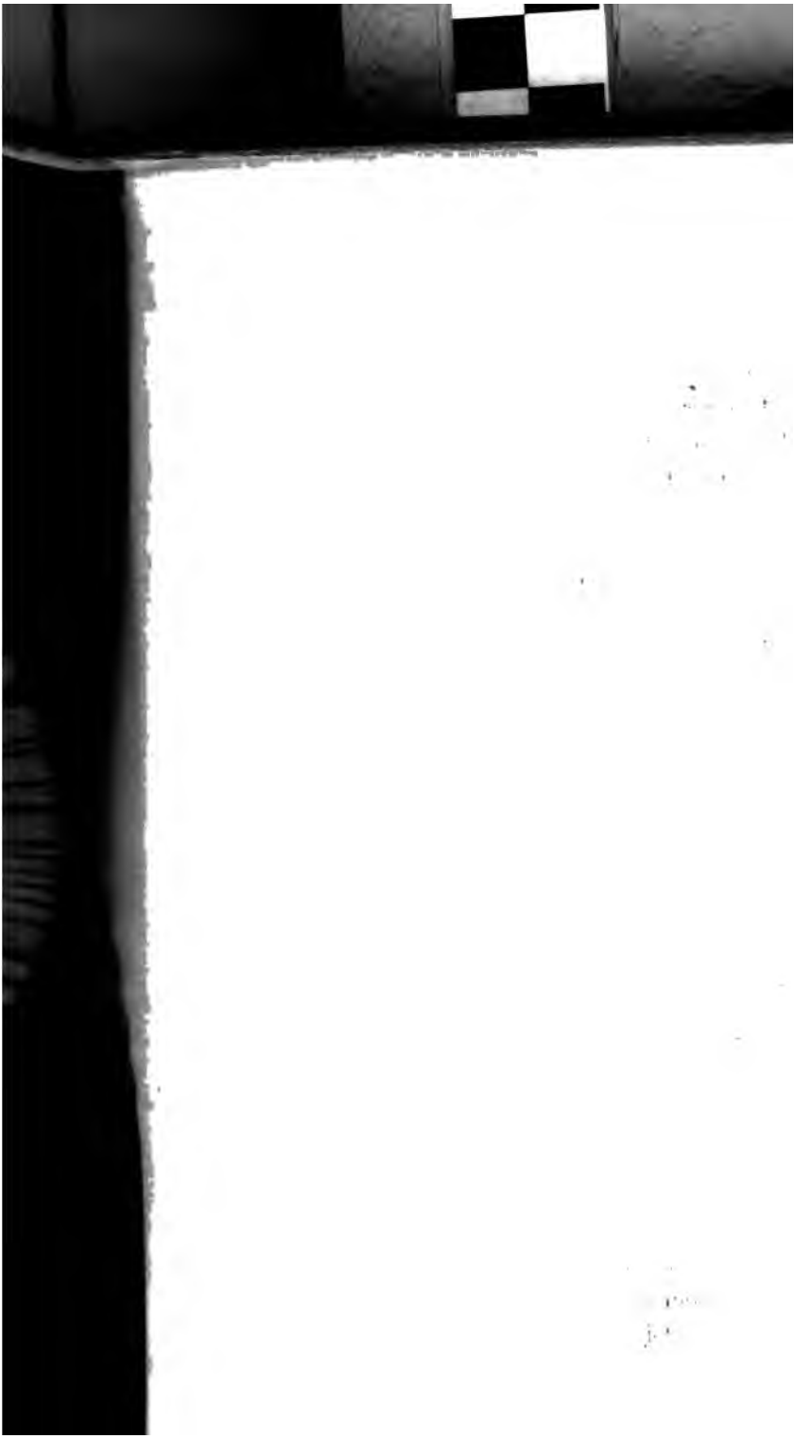
It is, however, always possible, or rather it is highly probable, that different parts of the corona may give different spectra.† The ascertained facts are these—that some parts of the corona do undoubtedly give a spectrum consisting in part of one bright line; that

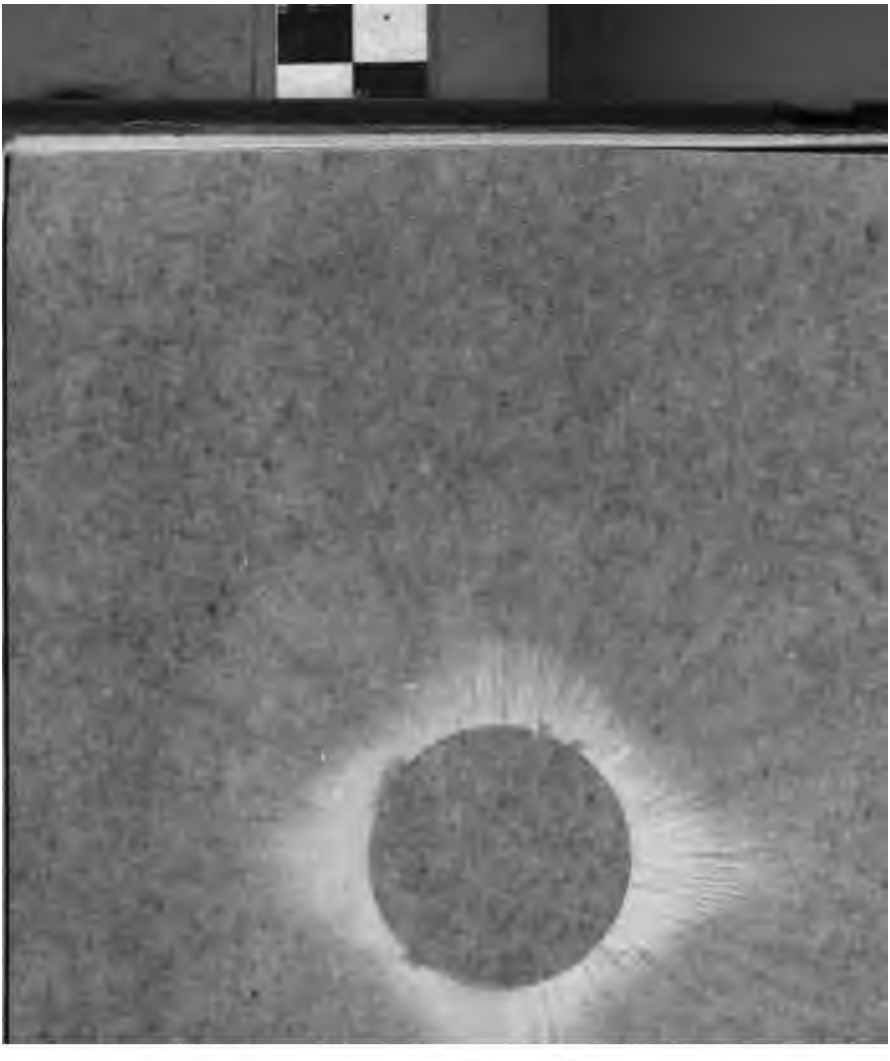
* Professor Young writing to me on March 10, 1871, says, 'Professor Pickering's spectroscope was not attached to a telescope at all. It was what I have ventured to call in an article which appeared in *Nature*, for Feb. 2, an *integrating* instrument, giving the *general* spectrum of the whole mass of light from the whole solar region, protuberances, chromosphere, and all, without any discrimination between them. The lines he saw were "near C, near D, and near E, the last the brightest." Lockyer held that this last was a mistake for F. I claimed it for 1,474, and Mr. Abbay's and Mr. Pye's observations in Spain prove my correctness. Pickering did not see F at all in 1869. (Pye you recollect found it very faint, and I do not at all understand how Abbay saw it, without, at the same time, seeing C and D_3.)

'Of course, in Professor Pickering's observation, then, the lines near C and D were due to the prominences, and were really C and D_3, while the line near E was the 1,474 Corona line.

'It would follow, of course, also, that so far as our American observations are concerned, there was no discrepancy whatever to be reconciled, except that the two faint lines which I *doubtfully* believed to be corona lines, were probably prominence or chromosphere lines and not coronal at all. I see, however, in a recent number of the *Academy*, a letter from Secchi, in which he speaks of Father Denza as observing *two* lines in the corona spectrum; so that possibly this spectrum may be variable like that of the aurora borealis, and I may have been right even in respect to the two faint lines.'

† This would accord well with what is observed of the spectrum of the aurora borealis. In the communication already referred to (note, p. 351), in which Mr. Birmingham describes his observation of three bright lines in the spectrum of the aurora, he says of the 'intense red of broad areas of light,' that 'there was here no line whatever to be detected.' And again he adds, 'the white light seen in some parts of the sky gave only the one principal line in the green.' We have seen also that the red line of the auroral spectrum is not commonly visible.





this line agrees in position with a bright line belonging to the spectrum of the terrestrial aurora; and that, so far as observation has yet gone, the spectrum of the corona contains no dark lines. We shall see that the observations made since the American eclipse confirm these results.

The direct observations of the corona as seen during the American eclipse were numerous and important. Mr. W. S. Gilman, jun., from whose coloured drawing of the eclipsed Sun Plate VII. is taken, writes thus respecting the appearance of the corona:—'The general outline of the corona was a trapezium, with the widest side to the south-east. There were also lesser projections on the four sides, as well as several small indentations. The longest masses of light coincided very nearly with the north and east points, at the middle of totality. Mr. Farrell also noticed the same peculiarity. The corona was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish white rays, issuing from behind the Moon. I detected no clouds in it. The exterior edge was very jagged in appearance, but did not possess a harsh outline, having, on the contrary, a soft blurred look. This was quite contrary to my expectations, as, from all the drawings of eclipses I had ever seen, I was led to expect a well-defined and truly circular halo of light, extending to an equal distance on all sides. Mr. Farrell described the appearance of the corona verbally as follows: "It was a silvery grey crown of light, and looked as if it was the product of countless fine jets of steam issuing

from behind a dark globe. Near the Moon's disc the light seemed almost phosphorescent."

'The small amount of light given by the corona,' he adds, 'is indicated by the remark that, "during totality the seconds on our watch-faces could not be read without the assistance of the lamps placed in the windows of the house to aid us at this juncture. The time could with difficulty be told by the larger hands."'

'Seen through green glass, the corona consisted simply of four or five prominences, extending around the Moon, smooth in their outline, shading off by imperceptible gradations, and rising to different heights, the greatest height not exceeding four or five minutes.'

Let us next consider Professor Eastman's account of the corona. 'I was considerably disappointed,' he says, 'with the appearance of the colour and brilliancy, as well as with the extreme contour of the corona. Most observers have described the colour as "pure" or clear white, and the light as very brilliant, while nearly all the published sketches represent the contour as nearly circular and regular, and the coronal rays as radial, and equally distributed about the body of the Sun. The colour of the corona, as I observed it, both with the telescope and without, was a silvery white, slightly modified in the outer portions by an extremely faint tinge of greenish violet; and I could not detect the least change in the colour, or in the position of the rays, during totality. The light of the corona was not brilliant—perhaps from the effect of haze—but appeared more like the pale light from the train of a

meteor than anything else that I could recall at the time. The corona seemed to be composed of two portions, both visible to the naked eye; in which, with the small instrument which I used, I was unable to trace any similarity of structure. The portion nearest the Sun was about one minute high, forming nearly a continuous band about the Sun, and appeared to be a mass of nebulous light, resembling in structure the most brilliant irresolvable portions of the Milky Way. Its colour was silvery white, and, like its density, appeared the same throughout its whole extent. The outer portion consisted of rays of light arranged in two different ways. In five places they were arranged into groups resembling star-points, composed of slightly convergent and radial rays, but elsewhere were disposed on radial lines. The colour of the bases of the star-points and of the radial lines was the same as that of the inner portion, while the outer portion of the points had a very faint greenish violet tint. The radial lines were the most prominent.' He adds that 'four of the star-points projected farther from the Sun than the ordinary radial lines, and gave the contour of the corona the form of a trapezoid. Between two of the largest protuberances scarcely any corona was observed.'

The observations of Mr. J. Homer Lane are interesting, because he had more particularly charged himself, he says, 'with the duty of watching for any possible low atmospheric limit, marked by anything like a regular boundary and superior intensity of

light very near the Sun's limb.' He does not appear to have succeeded in detecting any such signs of a boundary line limiting an atmosphere lying lower than the limits of the corona. 'I next turned my attention,' he proceeds, 'to the agglomerations of white light in the corona, and fixed upon two of those which were remarkable for their small size and the comparatively dense accumulation of light in them. These were situated about 80° from the vertex towards the right, as seen inverted in the telescope. In appearance they might well be compared to small telescopic comets, with tails of some length, but without a head, and with no distinct indication of a head at one end rather than the other. They were not far from radial in direction relatively to the Sun's centre—whether exactly so I did not remark at the time—but appeared completely isolated, and had their origin far above the limb of the Moon; so far at least that, though that part of the limb must have been approaching them, if their height above the Sun's limb remained constant, the approach did not attract my attention. I referred them two or three times to the profile of neighbouring lunar mountains, so far as to make it evident there was no such relative motion as must have been expected, had there been anything floating in our atmosphere. They manifestly belonged to the heavens, and I made no doubt were to be classed with the other parts or aggregations of white light in the corona. These two bodies I scrutinised closely for some time, and, as I suppose, to the end of totality. The form, dimensions,

and appearance of each, and their distance apart, were in constant review; but not the least change in either respect was seen. It would be in vain for me to try to estimate the length of time occupied with these objects. As to the distance between these two white comet-like objects, I judged, after the largest solar spot had been uncovered, that they would have included between their centres the nucleus of that spot, but not the penumbra. I had no means of taking any measures, nor would it have been easy to do so without sacrificing the scrutiny for the presence or absence of changes. In order, however, to reproduce as nearly as possible what I saw, I have laid down upon paper a circle of the same size as the Moon's outline in the focus of the object-glass—almost an inch—and holding this up to the light in the field of view of the same eye-piece used in the telescope, have tried to lay down with a pencil the dimensions and situation of the two objects. The following is the result I got in this way:

Length of each cometoid light	130″
Height of its origin above the Moon's limb .	100″ to 80″
Distance from centre to centre of the two . .	50″ to 40″

'Perhaps the height above the Moon's limb should be taken at about half a minute, shortly before the end of totality. The estimate of 50″ to 40″ here made for the distance apart of the two objects, may be considered entirely independent of the one first given referring to the solar spot. In that case the judgment comparing with the spot was checked by selecting a

pair of scratches on the blue screen glass in the field of view before the eclipse was over, whose interval was judged not far from equal to that of the two objects, and then bringing the solar spot between them.'

But perhaps the most important of all the observations made on the general aspect of the corona, during the eclipse of August 1869, were those made by General Myer, who watched the progress of the eclipse from the summit of White Top Mountain, near Abingdon, Virginia, 5,530 feet above the sea level. 'The point of observation,' he remarks, 'was sought with the view of placing ourselves as far as possible above the lower and denser strata of the atmosphere, and the smoke, haze, and obstacles to vision with which they are charged.' It is on this account, and because of the bearing of the evidence on the question of the effect which our own atmosphere produces on the appearance of the corona, that General Myer's observations are chiefly important. The telescopic observations are less interesting than they would otherwise have been, so far at least as the considerations we are now dealing with are in question, on account of the smallness of the field of view, which did not extend far beyond the prominences. General Myer remarks that, in the telescope, the corona or aureola exhibited a clear yellowish bright light closely surrounding the lunar disc, and fading gradually, with perhaps some tinge of pinkish green, into the line of the darkened sky. 'Upon this corona, extending beyond its brightest portion, the well-defined rose-coloured

prominences were projected at various points of the circumference.'* But it is when we turn to the description of the corona, as seen by the naked eye, that the characteristic peculiarities resulting from the position of the observer are recognised. 'To the unaided eye,' says Myer, 'the eclipse presented, during the total obscuration, a vision magnificent beyond description. As a centre stood the full and intensely black disc of the Moon, surrounded by the aureola of a soft bright light, through which shot out, as if from the circumference of the Moon, straight, massive, silvery rays, seeming distinct and separate from each other, to a distance of two or three diameters of the lunar disc, the whole spectacle showing as upon a background of diffused rose-coloured light. This light was most intense, and extended furthest at about the centre of the lower limb, the position of the southern prominence. The silvery rays were longest and most prominent at four points of the circumference, two upon the upper and two upon the lower portion, apparently equidistant from each other (and at about the junctions of the quadrants designated as limbs) giving the spectacle a quadrilateral shape. The angles of the quadrangle were about opposite the north-eastern, north-western, south-eastern, and south-western points of the disc. A banding of the rays, in some respects similar, has been

* Since both the prominences and the corona are luminous we cannot positively conclude, from this description, that a part of the corona really lay behind the prominences; yet it is well to observe how closely the description accords with this view, or, in other words, with the view that the corona is a solar appendage.

noted as seen at the total eclipse of July 18, 1860. There was no motion of the rays; they seemed concentric.'*

* General Myer's description of the general aspect of the sky and air when the total eclipse was in progress deserves to be added:—'The approach of the Moon's shadow,' he says, 'did not appear to be marked by any defined line, or the movement of any dark column of shade through the air. The darkness fell gradually, shrouding the mountain ranges and the dim world below in most impressive gloom. Our guides had been instructed to watch for the shadow, and to call to us at the glasses. They saw nothing of which to give notice. At the same time, and in vivid contrast, the clouds above the horizon were illuminated with a soft radiance; those towards the east with lights like those of a coming dawn, orange and rose prevailing; those northward and westward, as described to us by Mr. Charles Coale of Abingdon, Virginia, who was present, with rainbow bands of light of varied hues. I quote, in his words, a description written by him, as of interest in reference to the dispersion of light:—"The grandest of all to us, who had no astronomical ambition, or astronomical knowledge, to gratify, was the effect upon the clouds during the total obscuration. Those who have had the privilege of being upon White Top, and enjoying the westward scene, will remember the grand panoramic view of mountains, beginning on the northern and southern horizon, and stretching away to the west till they seem to meet, and will appreciate the scene that we now attempt to describe. Stretching along this semicircle of mountains in long horizontal lines, far below the Sun, lay light and fleecy clouds, as if resting upon their wings during the seeming struggle between the orbs above them. At the moment of the falling of the dark shadow, when naught was to be seen above but the stars and the circle of light around the Moon, these clouds became arrayed in all the colours of the rainbow, presenting an indescribable richness with their background of sombre mountain. To our vision, it was as if bands of broad ribbon, of every conceivable hue, had been stretched in parallel lines half round the universe."'

In a letter subsequently written to General Myer, on the subject of the remarkable colour-scene described above, Mr. Coale remarks: 'I was probably bordering on the extravagant (though not more so than is allowable in country journalism) in giving to the clouds "all the colours of the rainbow." I clearly remember, however, that there were distinct bands of pink, purple, yellow, orange, and fiery red, and each slightly tinged with different shades of its own colour. One of the bands had, I remember, to my vision, a slight lilac tinge. I do not remember to have

During this eclipse a more successful attempt was made to photograph the corona than on any former occasion. Fig. 90 represents the corona as photographed by Mr. Whipple, of Shelbyville, Kentucky. The four-cornered aspect is here distinctly recognised, and the

FIG. 90.

From a Photograph of the Solar Corona during the Eclipse of August, 1869.

probability may be inferred that, with a longer exposure, the rays would have been presented as seen by Gilman, Eastman, and others, if not as seen by General Myer. In the photograph there is, indeed, a sharpness of outline which might readily be interpreted by those unacquainted with the nature of photographic

observed any green or blue, but I do remember that the lower edge of the purple had a very faint blue tinge. All these resting against a dark background gave them an indescribably gorgeous appearance, the lines of colour seeming to be divided by stripes of black. They all lay in horizontal lines one above the other. My impression is, that those colours appeared at the moment the shadow passed from the lower edge of the Sun, though I am not positive.'

processes to imply the existence of a real boundary line separating this part of the corona from the part without. But, as a matter of fact, the sharpness of outline is due to peculiarities in the process of development. It may be recognised in the photographs taken at Ottumwa, although in them the corona has a much smaller extent. It is not noticed, however, in the photographs by Dr. Curtis, where the corona has about the same degree of extension; the reason being, that he employed special care in avoiding over-development of the negative. Hence no doubt whatever can remain that the sharpness of outline in the Ottumwa photographs, as also in Mr. Whipple's, implies no real limitation of the object photographed.*

* As some stress has been laid on this matter by those who advocate theories respecting the terrestrial nature of the corona, it may be well to present at length Dr. Curtis's statement respecting the erroneous interpretation of these photographic records. He says that he has read with surprise an extract from a letter, written by Dr. Gould to Professor Henry Morton, in which the former says: 'An examination of the beautiful photographs made at Burlington and Ottumwa, by the sections of your party in charge of Professors Mayer and Himes, and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the Moon, in the pictures made during the totality, is not the corona at all, but is actually the image of the sierra.' 'Dr. Gould proceeds,' says Dr. Curtis, 'to specify the points at variance between the corona as photographed and the same object as seen and sketched by him; and because the two representations do not correspond in feature, he infers that the objects depicted cannot be identical. This same argument would apply equally well to the "radiance" shown in my own photographs, since in them the phenomenon, though faint, agrees in outline with the similar object on the Burlington and Ottumwa pictures. Now, I cannot but believe that Dr. Gould is in error in imagining this aureola not to be simply the image of the more intense portion of the corona near the surface of the Sun. In the first place, the experience of this very eclipse has shown how guardedly all sketches and drawings of the appearances of totality

Such is a sketch of the evidence adduced up to the year 1870 respecting the solar corona. It appears

should be received, as affording an accurate record of either the shape, size, or position of the various objects. This is evident upon comparing the various sketches made by eye-observers of the protuberances and corona, both with each other and with the photographs, and observing the very great discrepancies manifest. Of course, it is not meant that accurate measurements made by a micrometer eye-piece in the telescope, or similar determinations of position-angle, cannot be relied upon, but, on the contrary, the argument is that *only* such are to be received as trustworthy, and that all *general* sketches and drawings made hastily during the few exciting minutes of totality, or from memory afterwards, form but a weak ground upon which to base an important scientific hypothesis. But positive proof in the question at issue is afforded by the very perfect photographs of the corona taken at Shelbyville, Kentucky, by Mr. Whipple, of the Cambridge expedition. Here we have a series of several negatives obtained by receiving the focal image of a six-inch object-glass directly upon the sensitive plate, and taken, with a wide range of exposure, from five to forty seconds. Of these the one exposed the longest (fig. 90) yields a splendid and unmistakable picture of the corona, representing it where the converging rays occurred, of a depth equal to a quarter of the Moon's diameter. Surely Dr. Gould cannot imagine the aureole of *this* photograph to be the chromosphere and not the corona; and yet *all* these pictures of Mr. Whipple's, and all of the Philadelphia expedition, and my own, agree perfectly in the features and position of the various irregularities in the outline of the corona, the difference in the representation of that object in the several photographs being solely one of extent and brilliancy. Dr. Gould adduces, as an additional argument in favour of his assumption, the observation that the long coronal beams appeared to him to be "variable," while the "aureole" photographed was evidently "constant" during the time of totality. This argument, however, loses some of its force when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration.'

Dr. Curtis then proceeds to consider how far Dr. Gould may have been led to found his opinion upon the circumstance that the 'aureole' in the Philadelphia photographs, 'while falling far short of the height above the Moon's limb attained by the corona as seen by the eye, yet appears of very great brilliancy, rivalling the protuberances in that respect, and comes to an almost abrupt termination a short distance above the solar surface.' He shows that these peculiarities must be

to me that although it does not suffice to answer all the questions of interest suggested by this imposing phenomenon, it yet leaves very little room for doubt as to the general characteristics of the corona. It will be well to weigh it carefully, before proceeding to discuss the bearing of later observations.

We are fortunately able to dispose very briefly of some of the theories respecting the corona which were suggested in old times. We need no longer inquire with close scrutiny into the theory that the corona is

regarded as in all probability simply 'photographic effects,' the prints of the photographs 'giving every indication that the negatives from which they were taken were strongly intensified after fixing.' 'This operation,' he adds, 'practised to give additional density to weak negatives would have, in this case, precisely the effect of increasing on the photograph the apparent brilliancy of the corona without adding to its extent. Moreover, that this excessive photographic brilliancy of the under portion of the corona should not be taken as a proof of any physical or chemical peculiarity in the actual object is quite conclusively proved by my own photographs, which, while showing about the same extent of corona as those pictures of the Philadelphia party that received the last exposure, yet represent it as a very feeble luminosity, fading gradually and imperceptibly into complete darkness, and this while the same photographs show the *protuberances* of great brilliancy. If this peculiarity of the Burlington and Ottumwa photographs had indeed any influence in leading Dr. Gould into the misconception into which I cannot but believe he has fallen, the circumstance affords but another example among many that I have seen, of the necessity that a critic, before attempting to draw scientific inferences from photographic representations, should himself become something of a photographer, else he will be very apt to fall into this natural error of ascribing effects wholly produced in the dark-room to physical characteristics of the object pourtrayed. And by a singular coincidence, evidence that Dr. Gould has not a practical acquaintance with the art would seem to be afforded in this same published letter, by his total misinterpretation of another purely photographic effect, viz., the apparent encroachment of the prominences upon the disc of the Moon as seen in the photographs. This curious appearance, instead of being due to "specular reflection," is wholly a dark-room phenomenon.'

due to a lunar atmosphere, because we now have abundant evidence that either there is no lunar atmosphere, or that at least no atmosphere competent to produce such a remarkable appearance surrounds our satellite. We know that two very definite results (to consider no others) must inevitably follow if the Moon had an atmosphere of even moderate extent. In the first place, the reflective power of such an atmosphere would cause somewhat more than one-half of the Moon's surface to be illuminated—precisely as, in the case of our own earth, the Sun is apparently raised by atmospheric refraction above the horizon of places lying beyond the hemisphere turned directly towards him. It is easy to show that under these circumstances, when the Moon is nearly new, her horns should extend somewhat beyond a semicircle. The fact that no such extension has been noticed suffices to prove that she has either no atmosphere or one of very limited extent. Again, the occultation of a fixed star by the Moon could not fail to be accompanied by evidence of the existence of any lunar atmosphere. Instead of disappearing suddenly, the star would be slowly reduced in brilliancy, and would appear to cling for a few moments to the outline of the Moon's disc. Since no such appearances are noted, we must reject the conception that the Moon has an atmosphere of appreciable extent, and with it the theory, which to Kepler and Halley had seemed attractive,* that the corona is a pheno-

* Halley mentions that contrary sentiments were entertained 'by those whose judgments he should always revere.' It has been supposed that Halley here refers to Newton.

menon due to the action of a lunar atmosphere on the solar rays.

Nor need we dwell on the theory propounded by Delisle, that the corona may be an optical effect due to the diffraction of the solar rays as they pass by the Moon, because Professor Baden Powell and Sir David Brewster have abundantly demonstrated that the effects due to such diffraction could not be discernible from the Earth.

We may thus limit our attention to two general theories (each admitting of special differences) which at present divide attention. One is the theory that the corona is a solar appendage; the other is the theory that it is a phenomenon due to the passage of solar light through our own atmosphere.

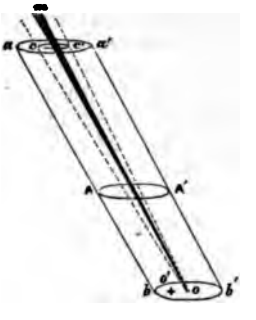
It will be seen that somewhat important issues depend on the selection we have to make between these two theories. For, if the corona be but a phenomenon of our own atmosphere, it is not worthy of more attention than we might give to the rays which stream through openings between clouds and form vast beams of light across the heavens. But if it be a solar appendage, then it is one of the most imposing phenomena the mind of man can dwell upon. Those long beams have (then) a real extension compared with which the volume of our Earth, nay even the volume of the Sun himself, sinks into utter insignificance; and that inner radiance which encloses the Sun on every side indicates a luminous region of inconceivably vast extent, while the problem suggested for our consideration by

the aspect of this region, and by the physical state of the material distributed through it, are of the most interesting character.

It will conduce perhaps to clearness of conception if we consider where that portion of our atmosphere is situated which, according to what may be called the 'atmospheric glare' theory, is illuminated by solar light during total eclipses. We can then inquire at our leisure by what path the solar light reaches this region of our atmosphere.

Let the observer be at O (fig 91) in the centre of

FIG. 91.



the Moon's shadow $b\,b'$, which forms an elliptical dark space on the Earth's surface. We need not concern ourselves with the shape of this ellipse, which will vary in different eclipses and at different stations. We need only note that in a considerable total eclipse the least diameter of $b\,b'$ will be greater than 100 miles. Now, let A b' represent a portion of the Moon's shadow-cone, forming within our atmosphere a figure not dif-

fering greatly from an oblique cylinder. Assigning to the atmosphere a height of about 200 miles this cylinder will have a shape such as A b'; but if we assign to the atmosphere an elevation of 500 miles,* then we should have to assign to our shadow cylinder the figure $a\ b'$. Now, let lines drawn from the observer's eye to the boundary of the Moon's disc enclose the black cone shown in part in O m, while lines drawn to the boundary of a circular corona extending one degree on every side of the Moon's disc during totality form the cone shown in part in O $c\ c'$. Both cones are shown well within the

* We are here considering, be it remembered, the atmosphere which is effective in reflecting solar light to the eye; and it will scarcely be admitted by most meteorologists that an atmosphere *of this sort* extends even to so great a height as 100 miles from the Earth's surface. Our best estimate (so far as this characteristic of the atmosphere is concerned) is undoubtedly that founded on the height of the twilight curve when observed from suitable stations, for this height depends on the very quality we are considering. Now Bravais, from a discussion of Lambert's observations of the crepuscular curve, deduced a height falling short of 100 miles, while his own observations, made from the summit of the Faulhorn, gave a height of about 66 miles. So far as the real extension of the atmosphere is concerned, we may accept the opinion of Dr. Balfour Stewart that observations made on the aurora supply the best means of forming an opinion. From such observations made in 1819, Dalton estimated the extreme height of the auroral arch at 102 miles. Sir John Herschel estimated the height of the auroral arch seen on March 9, 1861, at 83 miles. Observations of meteors afford another means of solving the problem A height greater than any of those above-mentioned has been deduced from observations of this sort. Lastly, polariscopic observations have led Liais and others to the conclusion that our atmosphere extends to a height of more than 200 miles from the Earth. The rarity of the atmosphere at such an elevation, assuming the law of diminution of density which prevails lower down to continue unchanged, would be altogether inconceivable. A quantity of air which a healthy person of average height could draw into his lungs at a single inspiration would suffice, when so reduced in density, to fill a sphere exceeding in diameter the orbit of Jupiter.

cylinder $a\,b'$, because as a matter of fact we find that the lines O a, O a' would contain an angle considerably greater than the angle c O c'.

Now, if the atmospheric glare theory is true, all the cone O $c\,c'$ in our atmosphere is illuminated at the time of central eclipse except only the core belonging to the cone O m. This is certain, because we *see* the Moon dark and the corona bright; so that we require O m to be dark and the remainder of O $c\,c'$ to be bright. Now, so far as undeflected solar rays are concerned, the whole region $a\,b'$ is in shadow. The light from the prominences can get into this region, and so perhaps can solar light deflected by some possible action at the Moon's surface. But the problem which the supporters of the 'atmospheric glare' theory have to solve is to get the light into the cone O $c\,c'$, growing brighter and brighter up to the very boundary of the dark cone m O (to correspond with the increase of the corona's light up to the Moon's limb), and there suddenly ceasing. This done they must show further that if another observer is stationed somewhere else within $b\,b'$ as at O', the cones formed by lines from *his* eye to the Moon's limb and the corona's boundary, are respectively dark and illuminated in exactly the same way—that is, they must show that the same regions of the air are at once illuminated and in darkness.

This may fairly be regarded as impossible.

Yet even if this could be demonstrated, much more would still remain to be done before the 'atmospheric glare' theory could be regarded—I will not say as

established—but as worthy of consideration. Until something of this sort has been done—and nothing of the sort has yet been attempted—we need not inquire how far those spectroscopic observations can be explained away which Professors Young and Harkness justly regarded as of themselves demonstrating the non-terrestrial character of the coronal light.*

It appears, then, that whatever view we are to form of the actual constitution of the corona, we can at least have no doubt that it is a true solar appendage. There may not be any closer bond of union between the material substance (of whatever sort) which emits the light forming the corona than exists between the nucleus of a comet and the comet's tail. But certainly the evidence seems to force on us the conclusion that a relation as unquestionable associates the corona and the Sun as that which compels us to regard the tail and coma as real appendages of a comet.

The corona thus viewed becomes one of the most important and interesting of all the phenomena of the solar system. We no longer have to deal with sun-

* It will be observed that the above considerations dispose of a modified form of the atmospheric glare theory more recently put forward, according to which some action on the Sun's rays as they pass near the Moon's surface may account for coronal phenomena. Such action does not render the 'atmospheric glare' theory at all more satisfactory. We have, if the theory is true, a certain region of our atmosphere illuminated and a certain other region dark, and the theory does not in any way explain how this comes about. Moreover, supposing it did explain the matter for one observer at one moment, the explanation would not avail to show how in the case of another observer that same illuminated region would be dark and that same dark region illuminated at the very same moment.

beams shining through our atmosphere, or with mirages in some lunar envelope, but with luminous spaces of inconceivably vast extent. Let us consider what evidence we already have bearing on the nature of this wonderful solar appendage. We shall then more justly appreciate the interest attaching to those efforts which are being made to gain fresh information.

The general aspect of the corona, as described in the preceding pages, does not suggest the idea that we have to deal solely with a solar atmosphere. Those radial projections are not the appearances we should expect to find in an atmospheric envelope. Nor again is it easy to understand how the irregular masses of light, the spots resembling hanks of thread in disorder, and other peculiarities of a like nature, can be accounted for on the theory that the corona is a solar atmosphere.

But this view of the corona may be regarded as disposed of completely by the nature of the lines seen in the spectra of the coloured prominences. We have seen (p. 298) that the gases forming the prominences probably exist at a comparatively low pressure—that almost certainly the pressure near the summits of the loftier prominences falls very far below the atmospheric pressure at the summit of Dhawala Giri and Mount Everest. Now if we supposed the extension of the corona limited to that shown in Whipple's photograph, we should yet, on the supposition that the corona is an atmosphere, deduce a pressure far greater than this, let our estimate of the tenuity of the upper

parts of such an atmosphere be what it (reasonably) may. But when we remember that under favourable circumstances the corona has been seen to extend to a distance very far exceeding the diameter of the eclipsed Sun, so that its depth (still regarding it as an atmosphere) would be more than a million of miles, or exceed sixfold the height of the loftiest solar prominences, we find ourselves compelled to reject the idea that we have indeed to deal in the main with a solar atmosphere properly so called.*

We conclude, then, that the matter (of whatever sort) existing where we know that the coronal beams extend, does not constitute a solar envelope. This being the case, we have to account for its subsistence or continuance in these regions, by some other conception than that of the combined forces of attraction and molecular or atomic repulsion which keep an atmosphere in equilibrium (statical or dynamical, as the case may be). Now there *may* be, and very probably there is, in partial question here the action of repulsive forces exerted by the Sun. Most unquestionably, as Sir John Herschel has pointed out, the Sun *does* exert repulsive forces, and those of a magnitude inconceivably enormous. But, in the absence of any exact knowledge of the condition of the coronal matter, or of

* These remarks relate to the theory that the whole extent of the corona is atmospheric. It is by no means impossible that an atmosphere of enormous extent surrounds the Sun, reaching far above the loftiest prominences. It will be seen, indeed, further on, that the phenomena of the corona can hardly be explained without assuming that such an atmosphere exists.

the nature and mode of action of solar repulsive forces, we must for the present limit our consideration to those forms of solar action which we can measure and estimate. We must inquire how matter swayed wholly or principally by gravitation might remain in the Sun's neighbourhood without being brought to his surface. We know that this can only happen when that matter is in motion with suitable velocity. Place any particle at rest at the distance of Mercury, and that particle would move off towards the Sun, and in the course of about fifteen days and a half it would fall upon that luminary. But endow the particle with Mercury's velocity, no matter in what direction (except directly towards the Sun's globe), and it will revolve around the Sun in an orbit having the same mean distance as Mercury's. It has become at once an attendant on the Sun, or we may say it is now a solar appendage. Take, then, a million, or a million millions, of such bodies and give them adequate velocities, even though in a million different directions, and the Sun forthwith has as an appendage a cloud of cosmical bodies, which will continue for ever, or for an indefinitely long period, as a cloud appendage. It will not be fixed—the relations even of its several parts will not be fixed; on the contrary, the cloud will shift and fluctuate, its members aggregating here and segregating there; but as a clustering solar appendage it will be permanent.

Now, if we regard the corona as consisting, not of one such clustering appendage, but of countless millions, severally insignificant perchance, but combining

to form a solar aureola of enormous dimensions and of inconceivably real magnificence, we are at least not imagining a new feature of the solar system. On the contrary, we have now for some time had abundant evidence that such an appendage must exist. In fact, we have simply been brought by the consideration of the corona to perceive an effect produced by a portion of the solar system which had already been recognised.

It is admitted that the Earth encounters each year more than a hundred meteor systems. It is known that each meteor system includes countless millions of meteors; it is known that, besides the meteors belonging to systems, the Earth encounters myriads of others which, because they have not yet been associated with any known systems, are called *sporadic*; and it is known that the total number of meteors actually encountered by the Earth in the course of a single year is upwards of 2,700,000,000 if we count only those visible to the naked eye, while the number mounts up to 146,100,000,000 if we include those shooting stars which can only be seen with telescopic aid.* In that fine hoop of space which the earth traverses each year —a hoop having a circular cross section of the relatively insignificant diameter of 7,900 miles—these amazing numbers of meteors are annually encountered and for the most part consumed as they pass through our air; and yet there is seemingly no diminution in

* These results have been simply deduced from Professor Newton's estimates for the hourly motion of the Earth, extended to include the Earth's motion round the whole of her orbit.

the supply. If we suppose that the space between the Earth's orbit and the Sun is supplied with equal richness, then undoubtedly we already begin to have evidence of some such solar appendage as we have supposed the corona to be.

But this is not all. We know that the meteor systems which cross our Earth's orbit have paths of great eccentricity, so eccentric in some cases as to carry the members of these systems much farther out into space than the orbit of distant Neptune. We know certainly, therefore, that the intersection of any meteor-orbit with our Earth is a mere coincidence; a coincidence which would be so unlikely, if there were but a few millions of such systems, that the laws of probability force on us the conclusion that there must be millions of millions of meteoric systems for each encountered by the Earth. We know again that, according to the laws of motion, the existence of multitudes of eccentric systems implies necessarily the aggregation of meteors in the Sun's neighbourhood.* On this account alone the average density of meteoric aggregation would be twice as great at half the Earth's

* This is true, notwithstanding the fact that in any single meteoric system, elliptical in figure, the average condensation (in long intervals of time) must be least near the Sun, owing to the relatively swift motions of the meteors there. For if we conceive the case of a vast number of eccentric meteoric systems in every variety of position, the diminution of aggregation due to this cause would be as the distances; but the increase of aggregation due to the volumetric diminution of concentric shells of equal thickness around the Sun with approach to him, would be as the square of the distance. Hence results on the whole an average increase of aggregation proportional to the diminution of the distance.

distance, three times as great at one-third the Earth's distance, and so on.

But further, meteors have been found to be associated with comets—in this way—that the only meteor systems whose orbits have been determined travel on the orbits of known comets. If we assume this relation, observed in the only instances we have yet had the opportunity of examining, to apply generally, then we must infer a yet greater increase of meteoric aggregation near the Sun. For it has been shown that cometic perihelia aggregate more and more densely the nearer we approach the Sun.

Yet further, it would seem from the researches of Leverrier into the motions of the planet Mercury, that within the orbit of that planet there must be an increased aggregation of matter. For he has shown that certain perturbations of Mercury's motion may be explained by the existence of several small planets travelling around the Sun within the orbit of Mercury; and as we have had no proof of the existence of even a single object of this class, and as the observed effects can be equally well accounted for by the supposition that myriads of much minuter bodies exist, we may fairly infer on this account alone that the neighbourhood of the Sun is richly peopled with minute cosmical bodies.

Yet once more, Baxendell of Manchester has proved that certain periodic meteorological phenomena, detected by him in the tabulated statements of the best observatories, can be explained by the theory that

there exists around the Sun a ring or zone or spheroid of matter (his theory does not insist, he says, on any special form) reaching to about that same distance from the Sun which Leverrier has assigned to the family of intramercurial planets.

Assuming that the meteoric families which undoubtedly exist, and undoubtedly become more densely aggregated with approach towards the Sun, do thus —as Leverrier's theory and Baxendell's would alike suggest—grow even yet richer at about that distance from the Sun to which the corona extends when most favourably seen, we have *this* also to further strengthen our belief in the resulting brightness of the solar appendage thus formed—that these meteors would be severally illuminated with inconceivable splendour on account of their nearness to the Sun. If we add to this, that those approaching most nearly to him would be rendered incandescent, if not vaporised, by the intensity of his heat, and that most probably electric discharges would take place between them * on account of the intense energy of the solar action, we have, I think, abundant reason for expecting that when the

* Under ordinary terrestrial conditions the electric discharge will not take place through a vacuum. But we have no evidence that an actual vacuum exists where the corona is seen. On the contrary, it is probable that multitudes of the minute bodies travelling past the Sun become vaporised, and so combine to form a moving vaporous region, whose constituent parts are continually changing as fresh matter arrives and as portions pass away to distances where they can resume their normal condition. Changes also occur, in all probability, which no physical researches yet made can explain, since we have proof of peculiar forms of action in the formation and rapid growth of cometary appendages.

Sun undergoes eclipse an aureola of splendour would be seen around him.

We have, then, two distinct lines of argument. We have been led by the consideration of the phenomena actually presented by the corona to the conclusion that multitudes of bodies too minute to be separately visible exist around the Sun; while we have been led by the consideration of what we know respecting multitudes of minute bodies actually travelling around the Sun to the conclusion that a corona or aureole of light would be seen around him during total eclipse. It seems clear, too, that all those peculiarities of the corona which have seemed to oppose themselves so obviously to other theories accord most perfectly with this. Save, perhaps, one only. If the corona is really crossed by radiating dark bars such as are shown in Mr. Gilman's picture, and described by several observers of total eclipses, then most certainly that phenomenon is not accounted for by the theory here put forward. The meteoric theory will account for a radial dark bar—by which I mean a bar directed in a straight line from the Sun—as an occasional phenomenon. But that such bars should be a characteristic phenomenon of the corona, or that in any single case the corona should be seen streaked with several such bars, is a phenomenon which nothing in the meteoric theory considered *per se* is calculated to explain.

Now this is a difficulty which must be faced by some better means than a mere attempt to negative the evidence. That erroneous observations are made from

time to time is unfortunately true; but when a characteristic phenomenon which does not seem likely to be merely *imagined*, is attested by trustworthy observers, a theory begins to wear a most questionable aspect which can only be supported by assigning those observations to illusion.

The difficulty in this case is that, even setting aside the objections (overwhelming as I think) which have been brought against all other theories but the meteoric one (not as the sole explanation of the corona, but as an absolutely essential part in any complete explanation), we do not find in any of these theories the means of explaining this particular phenomenon.

Certainly no theory involving the existence of a lunar atmosphere can aid us in this strait; because if the radial bars were due to the passage of the solar rays straight through such an atmosphere in lines touching the Moon's edge (and no other path would account for the observed phenomenon), then the extension of the lunar atmosphere necessary to account for the observed appearances would be about 200,000 miles, or the lunar atmosphere would extend nearly to the Earth.* We cannot for a moment imagine this.

It will be shown also, further on, that no sensible part of the corona's light can be due to the existence

* It is easy to see this by making a diagram showing the Moon and Earth, with the paths of the imagined rays extending along the boundary of the Moon's geometrical shadow, carrying the rays on until, as seen from the station of an observer on the Earth (and within the shadow-cone), they shall subtend an angle four or five times as great as that subtended by the Moon's diameter.

of scattered matter (cosmical dust) between the Moon and the Earth, *as well as* throughout the whole of that portion of space circled by the Earth in her course around the Sun.

Nor again can the 'atmospheric glare' theory explain this phenomenon, unless we suppose the Earth's atmosphere to extend nearly to the Moon's distance,—which is altogether incredible. Indeed, even if we admitted this, the bright rays between which the dark rays are seen ought to grow brighter and brighter with increase of apparent distance from the Moon, which is the reverse of what is actually seen.

It remains, then, that we should account for the phenomenon by the theory to which we have been led by other considerations. In fact, since the meteoric theory has been shown to accord so well with other phenomena, while it derives a negative strength from the obvious flaws in all the other theories, we are justified in accepting with a certain degree of confidence any explanation of these dark radial bars which the meteoric theory may point to. In other words, instead of feeling bound to explain these dark bars before admitting the meteoric theory, we may employ the meteoric theory to supply the explanation we require.

Now, there are two phenomena—one belonging to the solar system, the other to the Earth's economy—which seem likely to aid us in this matter. One is the appearance of comets' tails; the other is the aspect of auroral streamers. As respects the former pheno-

menon, it is to be remarked that the directive forces, whatever they may be, which cause the tails of comets to project from the Sun, reside undoubtedly in the solar globe, and act undoubtedly with very great energy on certain forms of matter near him. Hence, as we have abundant reason for believing that the corona is not free from a certain association with cometary matter, we need not be altogether surprised if we find in the corona evidences of the same sort of action that we recognise in the formation and projection of comets' tails. Yet again, as respects the second phenomenon, we have the striking evidence afforded by the spectroscope to show that a resemblance of some sort exists between the coronal light and that of our auroras; so that we are justified in tracing some resemblance, and even in conceiving that some association exists, between the long straight streamers which form so remarkable a feature of the aurora borealis, and those straight radial bars,* with dark intervening spaces, seen in the solar corona.

* It is worthy of notice that a difficulty exists in the relatively small section of the radial bars, both bright and dark; for this phenomenon would imply that the centre of that action to which these bars are due cannot have the dimensions of the solar globe which we are able to measure. It seems far from unlikely (since indeed we have other evidence corroborating such a view) that the central and more condensed portions of the Sun's mass may be the real seat of this intense repulsive action.

[This was written in 1870, and I have preferred to leave it unchanged in order to show that by a careful analysis of observed facts the results of later observations may be in part anticipated. What I have here and in the main text described as suggested by what had been noticed before the eclipse of 1870, was strikingly confirmed if not demonstrated by the observations made during that eclipse.]

There is a circumstance which seems to render this relation more striking in the fact that the only explanation one can readily conceive of the observed characteristics of the auroral spectrum seems to bring us again upon that subject of meteoric astronomy which has thus far stood us in such useful stead. For among the bright lines seen in the auroral spectrum is one agreeing in position with a line of iron, and it has been thought probable by Stewart, Ångström, and others, that the light of the aurora is due in part to electrical discharges taking place in the upper regions of our atmosphere. But how can iron reach those upper regions save from meteoric visitants? and what can be more likely than that iron does actually reach the upper regions of our air in this way, when we consider how largely iron enters into the composition of nearly all the meteoric masses which have been hitherto subjected to analysis?

But it may be reasoned that if this is indeed the case—if solar action in the upper regions of the Earth's atmosphere (or terrestrial action excited in some way by the Sun) can cause these electrical discharges—then solar action exerted directly on similar material in the other parts of the Sun's domain ought to excite a similar luminosity, and that therefore we ought at night to see some traces—faint, it may be, but still recognisable—of this particular form of phosphorescence.

This amounts, in fact, to the consideration, that the limits of the corona as seen during total eclipse ought

not to mark the real limits of the Sun's light-exciting action. And even supposing that but a small proportion of the coronal light is really due to this form of action—that is, to electrical discharges—it would still be likely that some signs of those meteoric systems whose illumination by the Sun has been here regarded as the cause of the corona should be seen beyond the observed limits of that aureola of light.

Here again it happens (and I know no surer test of the justice of a theory) that we have been led to see that a certain phenomenon *should* be manifested, which actually *is* a familiar phenomenon of the heavens, and which would most assuredly have required explanation if it had not thus been led up to.

For precisely in that region where we should expect to find a faint gleam of light—precisely where the known relations of the planetary scheme would lead us to look for an abundance of meteoric material, there appears that mysterious luminosity known as the Zodiacal Light. And just as our reasoning has led us to regard the meteoric appendage of the Sun—an appendage really extending far beyond the orbits of the most distant planets—as variable in configuration, however constant when regarded as a whole, so we find the zodiacal light varying from year to year in brightness, and extent, and position. Its light, again, presents that faint tinge of pink which has been recognised in the corona and forms so marked a phenomenon of the aurora. It has even been observed to fluctuate in brightness and to be traversed by flicker-

ings and coruscations—to thrill, as it were, responsive to mysterious influences, precisely as we should expect on the supposition that it is analogous to the aurora. It must be added, however, that although Ångström has announced that the zodiacal light gives the same spectrum as the aurora, recent observations by Liais appear to show that its spectrum is continuous.

I have said that if we were not led by our consideration of the corona to anticipate as it were the existence of the zodiacal light, we should have to explain this latter phenomenon. Let us view the zodiacal light apart for a moment.

We have a glow or radiance which is commonly seen along the zodiac,—that is, in the region of the sky where planets are to be looked for. This glow obeys all the usual laws observed in the motion of celestial bodies. It rises and sets precisely as the fixed stars and planets are observed to do. If we travel towards or from the equator, it is seen higher or lower, precisely as the part of a planet's path near the Sun's place would shift. It presents all those peculiarities, in fine, which force on the astronomer the conclusion that he has to do with an extra-terrestrial phenomenon, and a further peculiarity showing that it is a phenomenon specially associated with the planetary scheme.* It has therefore been regarded by every

* Space forbids my entering here into a consideration of the arguments by which all other theories of the zodiacal light may be negatived. In the *Monthly Notices* of the Royal Astronomical Society, November 1870, there is a paper of mine, showing by mathematical considerations of a very plain kind that the only admissible theory of the zodiacal

astronomer who has studied the subject with due attention, as indicating the existence of a lens-shaped region around the Sun within which cosmical matter is strewn with considerable profusion.

Now, regarding the zodiacal light in this way, and considering its general aspect when seen under favourable conditions, the conclusion is forced upon us that the density of aggregation of this cosmical material increases with proximity to the solar globe. For we see that the borders of the zodiacal light are very much fainter than the central part or core of the gleam. We see, again, that the light grows brighter and brighter towards the horizon,—that is, with proximity to the place of the Sun. And these relations are observed even in those countries where at certain seasons the zodiacal light is vertical, and where therefore the actual arc separating its base from the Sun's place is least at the time when the light is first visible after sunset or before sunrise.

The obvious conclusion is, that if the zodiacal light could be traced yet farther towards the Sun's place, this increase of lustre would continue, and that therefore all round the Sun there would be seen a luminosity

light is that same theory which I have here urged in explanation of the corona,—the theory, namely, that there exists around the Sun a region of meteoric matter continually changing in configuration and constitution, owing to the continual arrival and departure of individual meteors. Every peculiarity of the zodiacal light is in accordance with this view, and many of its features, as also many features of the corona, seem individually explicable on no other hypothesis; while assuredly no other theory can account for *all* the observed peculiarities of these remarkable phenomena of our system.

corresponding precisely with the observed aspect of the corona. So that *again* we are led by the consideration of a well-recognised feature of the solar system to the conclusion that the corona is a phenomenon to be expected when the Sun is totally eclipsed, rather than one whose appearance should be regarded as surprising and perplexing.*

Writing in November 1870, and summing up the evidence obtained up to that time, I made, in the first edition of this work, the following remarks:—'While the exact nature of the corona remains—and perhaps

* To the considerations above adduced, I may add some which are touched upon in a paper of mine which appeared in *Fraser's Magazine* for February 1870:—'There is one feature of comet's tails,' I there point out, 'which has long since attracted attention, and will remind the reader of the peculiarities common to the zodiacal light and the aurora. I refer to the sudden changes of brilliancy, the flickerings or coruscations, and the instantaneous lengthening and shortening of these mysterious appendages. Olbers spoke of "explosions and pulsations, which in a few seconds went trembling through the whole length of a comet's tail, with the effect now of lengthening now of abridging it by several degrees." And the eminent mathematician Euler was led by the observation of similar appearances to put forward the theory "*that there is a great affinity between these tails, the zodiacal light, and the aurora borealis.*" The late Admiral Smyth, commenting on this opinion of Euler's, remarks that "most reasoners seem now to consider comet's tails as consisting of electric matter" (that is, I suppose, indicating the occurrence of electric discharges), adding that "this would account for the undulations and other appearances which have been noticed—as, for instance, that extraordinary one seen by Chladni in the comet of 1811, when certain undulatory ebullitions rushed from the nucleus to the end of the tail, a distance of more than ten millions of miles, in two or three seconds of time." To this may be added the theory suggested by Sir John Herschel, that the matter forming the zodiacal light is "loaded, perhaps, with the actual materials of the tails of millions of comets, which have been stripped of these appendages in the course of successive passages round the immediate neighbourhood of the Sun."'

may long remain—a mystery, I know of few instances in which the general nature of a phenomenon has seemed more satisfactorily exhibited than in the case of the corona and zodiacal light. We have the strongest negative evidence against all other theories but one, and that one theory is confirmed by line after line of positive reasoning. To doubt what general view we should form of the corona and zodiacal light under these circumstances seems to me to savour—*not* of that wise caution which prevents the true philosopher from overlooking difficulties, but rather—of an inaptitude to estimate the value of evidence. As to details we may be doubtful. Other matter than meteoric or cometic matter may well be in question; other modes of producing light, save heat, electricity, or direct illumination, may be in operation in this case; and, lastly, there may be other forces at work than the attractive influence of solar gravity, or the form of repulsive force evidenced by the phenomena of comets. As regards, also, the true shape and position of the coronal and zodiacal appendage—and yet more as regards its variations in shape—we may still have much to learn. But of the general fact that the corona and zodiacal light from a solar appendage of amazing extent and importance, that they are not merely terrestrial phenomena, but worthy of all the attention astronomers and physicists can direct to them, it seems to me that no reasonable doubts can any longer be entertained.'

I have purposely suffered the reasoning with which this chapter was brought to a close in the first edition to remain almost untouched, in order that the results of later observations, as well as the theoretical considerations based upon such observations, might hold their due place in the history of this portion of my subject. The reasoning in the last twenty-four pages was based on observations made before the eclipse of December 1870; but that foundation was quite wide enough for its support. I do not find that any of the conclusions enforced above require to be abandoned—or even modified—in consequence of later observations. On the contrary, they have all been very strikingly enforced. But fortunately these observations have not availed only to make plain what had before been established. Had they done this only, they would have been wholly wasted; since scientific observations should have a nobler end than to supply simple proofs of facts already in reality demonstrated. All who heard of the laborious and costly preparations for the eclipse of December 1870 hoped that some new discoveries would reward the observers, and this hope was not disappointed. On the contrary, few eclipses have revealed more striking facts than were discovered on that little favourable occasion—the so-called Mediterranean eclipse.

First, let me sum up the already known facts, which received confirmation (unrequired but not uninteresting) during the Mediterranean eclipse.

The fact that the corona may be divided into two

distinct portions—first noted, as we have seen, by Plantade and Capiés in 1706—was fully recognised by the observers of this eclipse. Indeed, a somewhat remarkable stress was laid by some on this supposed discovery, until it was pointed out that in eclipse after eclipse the peculiarity had been noticed and described. As I wrote at the time, 'nothing in astronomy had been more thoroughly determined than the general uniformity of the coronal light up to a certain distance from the Sun, beyond which a rapid, but not sudden, degradation of lustre takes place;' so that 'Only that forgetfulness of past labours and their results which has too often characterised eclipse observation could have caused any stress to be laid on the recognition of so well worn a fact.'*

* The supposition that a discovery had been made led naturally to the suggestion of new nomenclature. Astronomers had hitherto been satisfied to distinguish by verbal description merely between the inner and brighter portion of the corona, and the outer fainter and radiated portion. Nor does there appear any necessity for assigning definite names to features which, after all, are portions probably of one and the same object. But the growing taste for sesquipedal nomenclature could not be satisfied, it would seem, until the inner part of the corona had been called the *leucosphere*,—a term singularly inappropriate as well as essentially hideous. Fortunately there were some who contended that the chromosphere (or, more correctly, the chromatosphere) really included this leucosphere. Consequently, the word leucosphere has not been accepted, and even the word chromosphere has to some extent lost favour. Technical words are sometimes absolutely necessary (for instance, we could not well afford to lose the word *photosphere*), but no new word was wanted to replace the good word sierra; still less was the word leucosphere wanted. As to *sierra*, it may be admitted at once that this word only designates truly the appearance, not the real nature, of the coloured envelope. But in this respect it corresponds strictly to the term 'prominences' for the upthrown parts of this envelope. Chromatosphere has no advantage on the score of accuracy of description; or

In the next place, the observations made by the American astronomers in 1869 were amply confirmed. It is difficult to understand why these observations had been questioned. They were indeed sufficiently striking, and they threw a quite unexpected light on the whole subject of the corona. As Professor Young remarked, the character of the coronal spectrum proved beyond all reasonable doubt that the light of the corona came in great part from self-luminous gaseous matter. This was not a result to be rejected, however, simply because it was remarkably interesting. Yet with a caution, neither complimentary towards the skilful spectroscopists of America, nor just in itself, their observations were treated as untrustworthy, or, at least, not to be accepted until confirmed by European observers. Fortunately the spectroscopic observations made during the Mediterranean eclipse were most satisfactory and conclusive. One of the first telegrams received in this country announced the complete confirmation of the American observations. Italian and English, as well as American spectroscopists, saw the bright-line coronal spectrum. The most complete evidence came, however, again from the American observers. I would dwell in particular on the observations and reasoning of Professor Young, respecting the bright line known as Kirchhoff's 1474. He tells us that of the four spectroscopes used by his

rather, it is at a great disadvantage, for in reality it suggests a theory which is admittedly incorrect. As regards the word *leucosphere*, attack is needless, since defence is impossible.

party, two were *analysing* and two were *integrating* spectroscopes. The former, it will be understood from what has been already stated (p. 140), gave the relative brightness of the several lines in the spectrum from a definite part of the corona or prominences, as the case might be; the latter showed the relative quantity of light corresponding to the several lines, when the whole of the corona supplied light to form the spectrum. Now Professor Young tells us that, in the analysing spectroscope, 'even during totality, the line 1474 can hardly be called conspicuous, while the line C blazes like a red Sirius,' and he considers that under the circumstances C is at least twenty-five, and perhaps fifty, times brighter than 1474. But in the integrating spectroscope the order of brightness is reversed: the line 1474 is so much brighter than the line C, that Professor Young assigns the numbers 100 and 72 to indicate the relative brightness of these lines respectively. It follows clearly that the line 1474 must be reinforced, when the integrating spectroscope is used, by the greater area whose light gives the 1474 line. 'The area of the region supplying the light whose spectrum shows the 1474 line, must exceed the area of the region whose light shows the C line, in the proportion which 100, multiplied by from 25 to 50, bears to 72, or from 35 to 70 times.' So that Professor Young, assigning to the coloured sierra and prominences an area equivalent to a ring a quarter of a minute high round the Sun, concludes that the self-luminous corona would be 'equivalent to another ring from 8′

to 14″ high.' We have then 'surrounding the Sun beyond any further reasonable doubt a mass of self-luminous gaseous matter, whose spectrum is characterised by the green 1474 line. The precise extent of this it is hardly possible to consider as determined, but it must be many times the thickness of the red hydrogen portion of the chromatosphere: perhaps on an average 8″ or 10″, with occasional horns of twice that height. It is not at all unlikely that it may even turn out to have *no upper limit*, but to extend from the Sun indefinitely into space.'*

* It should be added that Professor Young was able to establish in the most satisfactory manner the agreement of the line spoken of as Kirchoff's 1474 with that seen in the coronal spectrum. He says—'I have not [with the slightest reason to suspect the least want of correctness in the corrections made with the whole dispersive power of the prism. If any difference exist it must be that one-tenth of one division of Kirchoff's scale. There is no other dark line but 1474. there for with which this bright line can be supposed to agree.'

Professor Young suggests in 1869 that the bright line he had observed in the coronal spectrum indicated the existence of a new element in the corona. I must admit that I cannot find sufficient reason, however, for this conclusion. It is not indeed unlikely that elements unknown to us on the earth may exist elsewhere, but very striking evidence would be needed to establish of what it is that produces the actual existence of such elements in any special instance. In the present case the only positive evidence we have in this direction is the conclusion that the new line is not a very well-known chromatic—the most familiar to all of us—in question. It may be difficult to explain why only one line of the coronal spectrum is seen, and that not one of the strongest of the ordinary metallic line spectrum of iron. But it appears a somewhat daring solution of the difficulty to suppose that some new element is in question. Professor Young has, I believe, merely suggested the idea as one to be remembered among the possible solutions, but to this there can be no objection. It should be noted, as Professor Young himself remarks, that the brightness of the bright line varies, besides being one of the lines we first saw during the total eclipse, he also has recently ascertained its place. He has yet refrained from assigning

The Italian observers at Agosta saw two very brilliant bands in the spectrum of the corona. 'Father Denza,' says Secchi, 'executed the part assigned to him to admiration. He saw the protuberances clearly, and choosing the most brilliant part of the corona, he directed the spectroscope towards it; and whilst the assistant kept it fixed, he was able to distinguish plainly the spectrum of the protuberances from that of the corona. This last was continuous, and had two very brilliant bands; one in the green, near Fraunhofer's E, and another in the yellow-green. The shortness of the time, and the difficulty of calculating the scale, would not allow us to fix the lines in question with more precision.'

From this observation Professor Young is inclined to infer that different parts of the corona may give different spectra, precisely as is the case with the aurora.

The polariscopic observations were perplexing, and in some respects contradictory. They seem to show, however, that (1) a considerable proportion of the coronal light is reflected; (2) that *all* the reflection does not take place in our own atmosphere; and (3) that it is not such as would result if the Sun's light were

a name to the suggested element: so that he has in no respect departed from the strictly scientific course in this matter. It is to be hoped that others will hereafter be equally careful; for if every unexplained celestial spectrum is to be regarded as indicating the existence of a new element (or several), there will soon be a most undesirable addition to the nomenclature of elements. Dr. Huggins's researches have already supplied the material for the formation of many new names relating to supposed non-terrestrial elements in nebulæ, in comets, and in planetary atmospheres.

reflected from mere amorphous dust, but rather
though the reflection took place from surfaces of
crystalline nature.

But undoubtedly the most important results of th
eclipse were those obtained by the aid of photograph
The eclipse of December 1870 stands distinguishe
in fact from all other eclipses, as the first during whic
the corona was well rendered on glass. We have see
that Mr. Whipple had fair views in 1869, and a car
ful study of F. Secchi's photographs, taken in 186
shows the corona to be really present there—ver
faintly traced indeed, but in a manner which we ca
now understand. But the only real picture of th
corona as yet taken by photographic means is, beyon
all question, that obtained by Mr. Brothers, at Syr
cuse, during the last thirteen seconds of the totalit
at that station. Mr. Willard, in Spain, obtained a
excellent view of the inner part of the corona, bu
a stop or diaphragm cut off all the outer parts; s
that his picture, though of great value and importanc
(more particularly for purposes of comparison), ca
by no means be regarded as a view of the coror
as it really existed in the heavens at the time of th
eclipse.

The explanation of Mr. Brothers's success is to b
found in the fact that he adopted a new method
photographing the eclipsed Sun. Hitherto a telescop
had always been employed for the purpose; but M
Brothers employed a photographic camera, mounted c
the telescope-stand (as a matter of fact mounted on

telescope*) so as to be accurately driven by the clock-work. The object lens was a very fine one of thirty inches focal length and four inches diameter, lent to Mr. Brothers by the maker, Mr. Dallmeyer. It gave a brilliant image of the Sun about three-tenths of an inch in diameter.

Before proceeding to consider the result of Mr. Brothers's work with this instrument, it is necessary to notice that the work may be viewed in two aspects—first, as it relates to the question (which some regarded as still unanswered) whether the corona is a solar appendage; and secondly, in its bearing on the very difficult problem of the determination of the corona's real nature and constitution.

After what I have said in the preceding pages, and had urged in many places and ways before the eclipse expeditions sailed, it will be understood that I regard the bearing of the photographs on the first point as a matter of very slight importance.

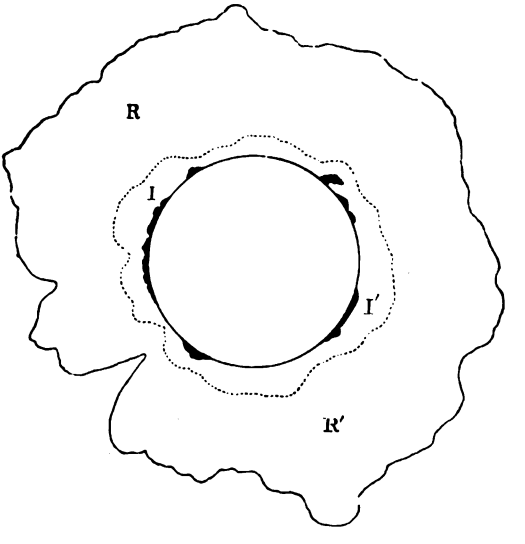
Still, so long as the doubts and difficulties just referred to continue to be urged, it is necessary to indicate carefully any new facts which may seem to supply a more *obvious* (though not a sounder) answer to them than any of the facts heretofore known. The evidence obtained during the eclipse of December 1870 was new and striking.

In the first place, that the teaching of the photo-

* The Sheepshanks Equatorial, belonging to the Royal Astronomical Society; the same instrument which was employed by Colonel Tennant in the spectroscopic analysis of the prominences in 1868.

graphs may be better appreciated, let us consid
the evidence obtained from direct observation. Th
evidence is in many respects more satisfactory tha
during former eclipses, because the attention of tl
observers was directed more especially to the deline
tion of the corona's shape. Owing to bad weathe

FIG. 92.



The Corona, as drawn by Lieutenant Brown. R R', the outer radiated corona; I I', the inner and brighter corona.

however, at some of the stations, the evidence fro
direct observations would have been incomplete—as v
shall presently see—but for the photographs of Willa
and Brothers.

In Spain the corona was well seen at several st

tions. It was described by most of the observers as generally quadrangular in shape, but with a remarkable V-shaped gap in the lower left hand (or eastern) quadrant. The accompanying picture, by Lieutenant Brown, agrees very well with the accounts of the other observers, except that the V-shaped gap is not here shown quite so large as some considered it to have been. Lieutenant Brown, when this picture was exhibited at the January meeting of the Royal Astronomical Society,* agreed (in answer to a remark to

* It should be noticed that when this drawing was exhibited Mr. Brothers had not yet reached England, nor had any description of his photograph been received. Indeed, his negative had been carefully packed away; and in a letter addressed to me (in the middle of January), he told me he was unable to describe the position of the dark rifts until he had unpacked his negative, so little was he aware of the anxiety with which his description and drawings were expected. It has been said (with unfair significance of tone) that Lieutenant Brown had had the opportunity of studying Willard's photograph during the voyage home from Spain. But no one who compares Lieutenant Brown's view with fig. 93 will believe that the former was based on the latter. Strangely enough, the very letter in which this charge (for the insinuation involves a serious charge) was made, stated that Willard's photograph should not be placed as in fig. 92, but inverted; differing wholly, therefore, from Lieutenant Brown's, but agreeing with Lord Lindsay's photographs. Now these objections cannot both be maintained. Nor, indeed, can either. The insinuation against Lieutenant Brown was an unworthy one; and there is not any evidence for it even as a possibility. None of the English observers in Spain had the opportunity of seeing Willard's photograph, which was, I know, new to several of them even so late as the January meeting of the Royal Astronomical Society. As to the other theory, it is undoubtedly true that the best of Lord Lindsay's photographs (the only published one) cannot possibly be brought into agreement with the photographs pictured in figs. 93 and 94, unless either it or both of those be inverted. It is not by the corona alone (very faintly shown in Lord Lindsay's photograph), but by the prominences, that this is shown. Now Mr. Brothers is absolutely certain that his photograph has not been inverted; and the American Professors

this effect) that his drawing did not quite adequat
represent the dimensions of the gap.

Let us compare with Lieutenant Brown's drawi
the following description of the corona, in the ' Engl

Young and Winlock have not understood that they have been n
taken in supposing that Willard's photograph is also correctly pla
as in fig. 89. Their comments also and the comments of many oth
on the agreement between the two photographs have been publis
for many months without eliciting any correction from Mr. Will
himself. It may be fairly believed then that Mr. Willard's opinion
to the true position of the photographs agrees with that of Mr. Brothe
Further, the opinions of Mr. Willard and Mr. Brothers must be
garded as confirmed by the silence of their respective assistants. (
Fryer, who worked with Mr. Brothers, is an exceedingly skilful a
well-practised astronomical observer, as is Mr. Brothers himself;
that it is impossible to suppose they could misapprehend a matter
pending only on the position of an instrument with which both w
perfectly familiar.) Against this evidence we have the opinion of L
Lindsay and his assistant, Mr. Davis, that their position for the phc
graph is the correct one. Lord Lindsay used a Newtonian reflector
excellent construction, by Mr. Browning—an instrument admira
adapted for the work of celestial photography; but nothing is ea
than to misjudge the way in which a photograph taken with such
instrument is to be viewed, in order that it may correspond with
actual aspect of the object pictured. The very circumstance which c
stitutes the great convenience of Newtonians (both for observation
photography) illustrates this; for the observer can look horizontall
the image of a celestial object situated anywhere in the heavens. Th
is reflection with inversion at the great mirror, reflection again w
inversion at the small mirror, and then the usual inversion of the r
in passing through the eye-piece. Now, remembering that Lord Li
say had had very little practice with his instrument, and was a c
parative beginner in astronomical observation, and that Mr. Davis
had even fewer opportunities, while they worked so rapidly during
eclipse as to take no less than 9 photographs in 2¼ minutes, can i
doubted that the error lies with them, and not with Messrs. Will
Fryer, and Brothers? Apart, however, from these considerations,
extended zone of prominences disclosed on the west of Mr. Brothe
photograph shows demonstrably that he has his picture properly pla
for it was taken at the end of totality, or just before the Sun's wes
edge was about to reappear.

Mechanic' for January 27, 1871 (the description was supplied by the special correspondent of that journal): —'The *corona proper*, or *glory*, or *radiated corona*, as it is variously called, extended a distance of almost the Moon's diameter from the Moon's limb, but not equally in every direction. It had a greater extension in four directions, at the extremities of two diameters at right angles to each other, so as to give it the shape, roughly speaking, of a square with rounded corners. It was broken in parts, and notably by one decided V-shaped gap. This was observed not only by our party, but at three stations—San Antonio, Xerez, and La Maria Louisa—which form a triangle, each of whose sides is five or six miles in length.'

In Sicily the direct observation of the corona was not so satisfactory as in Spain. Most unfortunately, neither Mr. Lockyer's party nor the party with Professor Roscoe on the slopes of Etna obtained any view at all of the corona. Professor Watson made an excellent drawing of the inner corona, and noticed that the outer was marked by dark rifts. The drawing is highly important, as will presently appear; but it does not afford any information bearing on the nature of the outer and radiated corona.

So much for direct observation. We have clear evidence that at Spanish stations, separated by five or six miles, a certain well-marked feature was seen, and this evidence is not directly contradicted by any observations made at Sicilian stations. When we consider the photographic results, however, all doubts which

might be suggested by the absence of any positive evidence from Sicilian stations, in favour of the V-shaped gap, are at once removed.

Fig. 93 is a representation of Mr. Willard's photograph, taken at Cadiz; the limit of the corona, where it extends farthest from the Moon's edge, being due to a stop or diaphragm. Fig. 94 is Mr. Brothers's photograph, taken at Syracuse; the corona is not limited, the image of the Moon having been but three-tenths of an inch in diameter on a five-inch plate. It is to be noticed also that Mr. Willard's photograph, being taken with a telescope, required a much longer exposure—in fact, it was exposed for a minute and a half, or nearly the whole time of totality. Mr. Brothers's photograph, on the other hand, was one of a series of five. It was taken during the last eleven seconds of totality, being exposed for eight seconds only. The sky was partially obscured during the earlier part of the total eclipse, but the plates were exposed according to the order planned before the eclipse took place; and with the following results :—

	Time of exposure	Results
First plate	3	blank
Second ,,	18	fair
Third ,,	30	slight
Fourth ,,	15	fair
Fifth ,,	8	good

The first and third plates may be regarded as failures (owing, of course, solely to clouds); the second and fourth, though not judged by Mr. Brothers himself to be so well worth publishing as the fifth, were still well

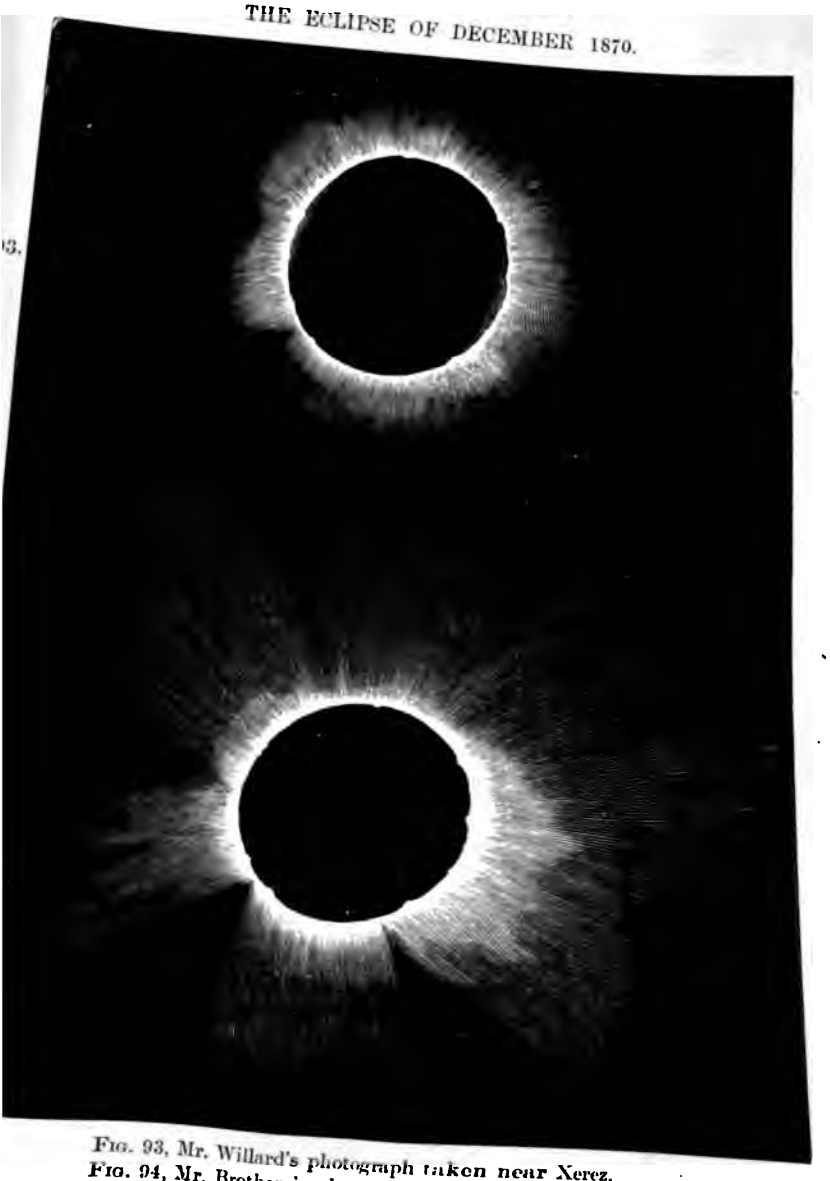


FIG. 93, Mr. Willard's photograph taken near Xerez.
FIG. 94, Mr. Brothers's photograph taken at Syracuse.

worth studying, and will be presently referred to as affording independent evidence of great importance. The fifth is pictured, as well as a woodcut can picture it, in fig. 94.

It is scarcely necessary for me to indicate the close agreement between these two photographs, or how closely both agree with the recorded results of direct observation. But it must be noticed that the woodcuts do not accord nearly so well as the photographs themselves, which, when reduced to the same scale, are absolutely identical as respects the parts of the corona common to both. The figure of the Moon has been impaired in Mr. Brothers's photograph, owing to the swaying of the camera by the wind. The range of prominences and sierra, which had just come into view along the western edge of the Moon, has 'eaten away' a portion of the Moon's disc on that side; but the corona (whose actinic energy is far smaller than that of the prominences) has not been affected by this circumstance, save in so far as its extension on the western side has been slightly increased by the removal of its base-line eastwards. The notches or gaps round both discs show the places where prominences have by their great actinic energy eaten into the photographic representation of the lunar disc; it will be noticed that their position removes all doubt as to the identity of the coronal rifts. Again, since Mr. Willard's photograph was the result of an exposure lasting during nearly the whole continuance of totality, it is clear that the rifts were not variable during totality. As

Mr. Brothers remarks :—'If the great rift had not been persistent, and only became visible during the last twenty seconds of totality at Cadiz, it could not have been shown at all, as the picture would have been "burnt out" at this part, and no dark space would have been shown.'

It is demonstrated then,—first, that a certain well-marked feature existed in the corona at stations so wide apart as Cadiz and Syracuse; and, secondly, that during the whole continuance of totality at a certain station this feature remained unchanged. The first fact proves incontestably that the V-shaped gap was not caused by any peculiarity in the atmosphere, since we cannot conceive that some peculiarity in the air over Cadiz chanced to have its exact counterpart in the air over Syracuse. The second fact proves as incontestably that the dark rift cannot be due to the passage of the solar rays along lunar irregularities, and thence through any matter (meteoric or otherwise) on our side of the Moon, since any dark rifts so caused would have rapidly changed in shape during the progress of totality.

It cannot be wondered at, therefore, that when Mr. Brothers's photograph was exhibited and described at the meeting of the Royal Astronomical Society, on March 10, 1871, no voice was raised in favour of the atmospheric theory of the corona.

At the same meeting two letters were read which will interest the reader.

One was addressed by Sir John Herschel to Mr.

Brothers, and is specially interesting as one of
latest of that great astronomer's comments on the fa
of his favourite science. The portion quoted ran
follows:—

'Assuredly the decidedly marked notch or bay
both photographs, agreeing so perfectly in situat
(marked so definitely by its occurrence just oppos
the middle point between two unmistakable red p
minences) is evidence not to be refused of its ext
atmospheric origin, and *almost* conclusive of its pro
mity to the solar globe. I see nothing which gi
me the idea of rays or streaky radiation such as t
figure in the "Washington Observations" (Plate VI
which, if it *could* be believed, would point to lu
mountains as the origin of the dark spaces, and br
the whole phenomenon within the distance of the lu
orbit. A terrestrial atmospheric origin is quite out
the question.'*

* I have preferred to give this quotation precisely as it stand
Mr. Brothers's paper. But, as a matter of fact, the *negative* does s
very distinct traces of streaky radiation. The explanation suggeste
passing (and as not really required) by Sir J. Herschel is not forced u
us however, since another (not noticed at this time by Sir J. Hersc
is available. We have seen, too, that the long exposure of Mr. V
lard's photograph disposes altogether of the lunar explanation. Bu
will be well for me to quote in this place from two other letters
dressed to myself by Sir John Herschel on this very subject. 'I ha
Sir J. Herschel wrote, 'like yourself been examining Mr. Broth
photograph, as compared with the American one—Syracuse with Ca
The coincidence of the notch in the corona with the middle point of
interval between the unmistakable prominences in both is quite
vincing. The corona is certainly *extra* atmospheric and *ultra* lu
(the italics are his). 'That is, without prejudice to *some portion* of
light being derived from cosmic dust in the neighbourhood of the E
and Moon, and intermediate between the Moon and Sun.'

The other letter was addressed by Dr. Balfour Stewart to Mr. Brothers, and relates to the effect of atmospheric glare in increasing the seeming extent

The other letter of Sir J. Herschel's relates to the question of the illumination of cosmical matter between the Earth and Moon. It had been suggested by Dr. Oudemann, that the coronal rays may be so explained, and Sir J. Herschel had expressed, in a letter to myself, a qualified acquiescence in this conception, which was supported also by Dr. De La Rue, at the January meeting of the Royal Astronomical Society. I ventured to point out certain objections which seem fatal to this theory (see p. 381); amongst other objections, the one illustrated in fig. 91. Here *e m* represents the space (towards one side of the

FIG. 95.

Illustrating the main objection to Oudemann's theory of the coronal rays.

Moon) whence the illumination producing one part of the coronal radiance is supposed, according to this theory, to proceed. Now, whatever light comes from this space *e m*, much more light must come from the space *m s s'* (*s* being the Sun), which is supposed by the theory to be also filled with cosmical dust,—since we cannot otherwise account for the existence of such dust within the space *e m*. For the distance *m s* exceeds the distance *e m* about four-hundredfold, and matter within the space *m s* would also be more brilliantly illuminated than that within the space *e m*; so that light from the former source, or from beyond the Moon, would altogether overcome light from the space *e m*. In answer to this, Sir J. Herschel (with the generosity familiar to all who ever had occasion to consult him) replied:—'In what I wrote about the cosmic dust, I did not take into consideration the much more intense illumination of the molecules near the Sun than of those near the Earth, which (quite apart from any question as to their possibly greater frequency in his neighbourhood) would give an immense predominance to their influence in producing coronal light. In this you are perfectly right.'

and importance of the corona. Dr. Stewart, in this letter, presents in a very striking manner certain considerations to which I have called attention in the Monthly Notices for March 1869. 'In the case of the uneclipsed Sun,' he writes, 'we have the well-known luminosity surrounding his disc, caused no doubt by atmospheric glare, so that we may be said to have sunlight *plus* glare; when, however, the Sun is totally eclipsed, the light which we see is derived not from the body of the Sun himself, but from his appendages, and the atmosphere acting in the same way upon this light as it did upon that of the uneclipsed Sun, we have a complex phenomenon before us consisting of solar appendages *plus* their atmospheric glare. Now I see no reason why the proportion between the real light and the glare should not be the same in both these cases; that is to say,

$$\left.\begin{matrix}\text{the light of the}\\ \text{Sun . .}\end{matrix}\right\} : \textit{its}\ \text{glare} :: \left\{\begin{matrix}\text{light of the solar}\\ \text{appendages .}\end{matrix}\right\} : \textit{their}\ \text{glare.}$$

I think I am right in stating two things with regard to your photographs. In the first place, the details of the luminous appendages surrounding the Sun are well represented. Secondly, there is little or no photographic effect over the centre of the Moon's disc. Judging from the photographs of the Sun himself taken by the Kew heliograph, we may conclude that when these pictures are not overdone, but give the various details of the solar disc, the exposure has been too small to give us the faintest trace of the atmo-

spheric glare which surrounds the Sun. Making use of the proportion above stated, I should conclude that a photograph of the solar appendages, not overdone, but presenting like yours the details of these appendages, would not present a perceptible trace of their atmospheric glare.'

'In the next place, the atmospheric glare due to the light from the eclipsed Sun should be thrown inwards over the Moon's disc as much as *outwards* beyond the true corona; the light, therefore, which reaches us in a total eclipse from the centre of the Moon's disc, and which may be partly due to earthlight reflected from the Moon, may be safely taken as somewhat exceeding that which can possibly be due to atmospheric glare; and inasmuch as in your photograph there is very little effect in the centre of the Moon's disc,* I am led to think that very little of the result obtained can be due to glare. I have here confined myself strictly to your photographs, but the principle laid down is applicable to all kinds of observation; and I must confess that I cannot at the present moment see why the streamers, if they are caused by the atmosphere,

* Some light was certainly received from the direction of the Moon's disc, because when the spectroscope was directed towards the Moon's centre the bright coronal lines were seen. A singular stress was laid on this circumstance by those who had once advocated the atmospheric glare theory,—as though the result had been unexpected, and confirmed their views. But so far back as March 1869 I had pointed out that the light from the solar appendages seen during totality must illuminate the air towards the Moon's place; and clearly, the recognition of a non-solar spectrum in that direction confirmed my statement that no direct sunlight can reach the air which lies in this direction.

should invariably shoot outwards, and never venture to trespass upon the Moon's disc.'

It should be added that the greater extension of the corona on the western side (in Mr. Brothers's picture) seems to be in no way connected with the Moon's motion, as was at one time suggested. For the same peculiarity is recognised in his second and fourth photographic plates (the only other good pictures, see p. 402), whereas the extension in the second plate should have been greatest on the eastern side if that were the correct explanation.*

I am thus led to consider what I hold to be far the most important result of the eclipse expeditions of December 1870.

When the photographic records, the best drawings,

* It will be noticed that the American photograph (exposed during nearly the whole continuance of totality) shows a greater extension of the bright inner corona on the same side. The best of Lord Lindsay's photographs (the only one published, as already mentioned) shows the inner corona brightest on the eastern side; and as that photograph was taken near the commencement of totality, some stress was laid on the fact by the advocates of Oudemann's theory (see p. 381). It seemed clear by comparing Lindsay's view with Brothers's that the light of the corona had passed from the eastern to the western side of the corona, as the Moon passed over the Sun's disc from west to east. But, unfortunately for this argument, it has now been proved beyond all possibility of question, that either Lindsay's photograph, or else Brothers's (and Willard's), has been inverted. I have shown in the note on p. 400, that the former view is altogether the more probable, or rather is practically certain; but for the present purpose it matters nothing which view we take. Whether we invert Lord Lindsay's photograph or Mr. Brothers's, we bring the brightest part of the corona to the same side in both photographs. So that the eccentricity of the corona was of the same character at the commencement as at the end of the totality—a circumstance disposing completely *per se* of Oudemann's theory (untenable, as we have seen, for other reasons).

the verbal descriptions, and the spectroscopic observations are carefully compared together, two relations are revealed which, whatever their interpretation, are undoubtedly full of meaning. They may be thus expressed:—

(1) *Where any great gap or rift appears in the outer or radiated part of the corona, there a depression is seen in the inner and brighter portion.*

(2) *Where the inner portion of the corona is depressed, there the coloured prominences are wanting and the sierra is very shallow.*

The former relation is very markedly shown in Lieutenant Brown's drawing, fig. 92 (and he referred to it as a fact he had specially noticed), and the Spanish and Sicilian photographs (figs. 93, 94) illustrate the relation in a very striking manner. In confirmation of the second I may quote Professor Roscoe's words respecting Mr. Seabroke's spectroscopic views of the prominences, and Professor Watson's drawing of the corona:—'On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences were seen in Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the Sun's limb.'

Of course the two relations involve an agreement

between the greatest extension of the outer corona and the regions richest in prominences. Mr. Brothers distinctly notes this agreement; for he says, 'there can be no question that there was more coronal light on the west side of the Moon than at the other points;' and then he calls attention to the fact 'that the prominences are more numerous on the side where the corona is brightest.' To this I may add Father Secchi's description of the western range of prominences. He tells us that towards the close of the totality (when the western prominences were disclosed) the protuberances formed a beautiful small semicircular corona, 'full of the well-known rosy jets, of admirable shape and beauty.'

Now, at first sight, one might be led to recognise in these relations some evidence for the atmospheric glare theory—in that modified form of the theory which makes the outer corona due to atmospheric glare produced by the inner corona and the prominences. Indeed Mr. Lockyer adopted even so late as August 1871 the following line of reasoning:—'The outer corona was observed to have a rosy tinge over the prominences, and the spectrum of the prominences was detected many minutes above them, as well as on the dark Moon. It could not have got this colour *at the Sun*, for its intrinsic colour was green, and the red light of the hydrogen supplied at the Sun was abolished altogether—was absorbed—and could only reach the corona *at the Sun*, so to speak, as dark light.'*

* From a paper read before the British Association in August 1871.

Here the reasoning as to light is sound, except as respects the conclusion that the intrinsic colour of the corona is green—a result by no means necessarily to be inferred from the green colour of the bright line in the coronal spectrum.* But a brief consideration of the geometrical relations involved shows that the redness of the coronal light over the prominences cannot possibly be explained by the atmospheric glare theory. The part of our atmosphere which lies towards the rose tinted extensions of the corona is not more illuminated by the corresponding rich prominence region than those neighbouring parts of the atmosphere which do not show this rosy light. If the rich prominence region were the source of the red tint, there could be no reason why there should be an extension in that particular direction, and not in others. We have, in fact, in the above reasoning a repetition, but in a modified form, of the misapprehension which characterised the form of the glare theory dealt with in pp. 408, 409.

It is only necessary to consider a rich red prominence region as a source of light illuminating our atmosphere, to perceive that the law according to which various parts of the atmosphere would be illuminated, must utterly preclude anything like radial extension from the region towards which the eclipsed Sun lies. There

* The light concentrated on a bright line may be far less in quantity than light forming a continuous spectrum, or a spectrum of broad bands. The light of the aurora, for instance, has sometimes shown a green line as the most conspicuous line, even when the colour of the auroral curtain has been markedly red. Similarly, the zodiacal light has a red tinge, yet the spectrum shows a green line.

would also be a gradual *increase* of brilliancy (instead of diminution) with distance from the Sun's place.

We are bound then to find an explanation of the phenomenon in some solar relations. In fact, we find ourselves brought by a new line to the difficulty already dealt with at p. 380, and considered as almost necessitating the idea of some form of radial repulsion exerted by the Sun. The meteoric theory *per se*, as pointed out at p. 380, will not avail to explain the straight radial bars and streaks seen in the corona; neither will it serve to interpret the relations we have now to consider. Regarding the prominences as phenomena of eruption, the occurrence of great solar eruptions cannot affect meteoric or cometic matter travelling myriads of miles above the regions of outburst. We cannot suppose for a moment that a sufficient superiority exists in the light-giving or heat-giving powers of the prominence regions to explain so marked a difference. Nor is it easy to suppose that some special form of electrical or magnetical action exerted above the prominence regions is the true explanation of the peculiarity. That electricity is involved in some way in the production of the coronal light may well be believed; and further, it is far from improbable (as indeed has already been suggested) that there is some special electrical action in progress above the prominence regions. But to explain a coronal extension by the assumed extension of this electrical action, is to explain one problem by another of greater difficulty. The sole explanation which seems available is so startling,

that at first sight many will be disposed to reject it on that account alone. It is this—that *a portion of the coronal light is due to the ejection of matter from the Sun, and either chiefly or wholly from those bands of the solar surface which we have thus far termed the spot-zones.*

Let it be distinctly understood, however, that this theory (in favour of which I propose to cite further evidence) is not to be regarded as intended to displace the meteoric theory, or even the theory that a solar atmosphere of extreme tenuity exists as far as, or beyond, the observed extension of the corona. On the contrary, we may be led by the emission theory to attach more weight than heretofore to the theory of a solar atmosphere extending very far from the Sun; while the meteoric theory, as accounting for *some* slight part at least of the coronal light, can scarcely but be regarded as demonstrated.

The first and most obvious objection to the theory that matter is projected from the Sun to such vast distances as the theory requires, resides in the enormous initial velocities which such ejection would imply. From what has been shown in Chapter II., it will readily be seen that a velocity of projection of about 300 miles per second would be required to carry matter from the Sun's surface even *in vacuo* to the observed limits of the corona. But as such matter, when first ejected from the Sun, must necessarily be in the state of vapour, and as it has to pass through the Sun's vaporous atmosphere (which close by the photosphere

must be of no inconsiderable density), there must be enormous retardative effect, and consequently a much greater initial velocity would be required.

We have, however, in the jet prominences indisputable evidence of velocities of ejection as fully beyond our powers of conception as the enormous initial velocities just referred to; and I think these prominences supply strong reason for believing that movements of far greater velocity must take place within the solar orb. The jet prominences quite commonly reach to a height exceeding 50,000 or 60,000 miles, and some of them have been 80,000 or 90,000 miles in height. Now to propel a projectile *in vacuo* to a height of 80,000 miles, an initial velocity of about 140 miles per second would be required. But the erupted matter is not a solid projectile flung through empty space, but glowing hydrogen (and other vaporous elements) flung through a hydrogen atmosphere. Now a cannon ball, though propelled with a velocity of less than a third of a second per mile, is yet so retarded by atmospheric resistance, that its range is a mere fraction of that calculated on the supposition that there is no resistance. It will be understood how enormously the initial velocity of these hydrogen projectiles (so to describe them) must exceed that inferred from the hypothesis of an ordinary projectile travelling *in vacuo*. So that it cannot but be regarded as probable (though it may be perfectly inexplicable) that the glowing hydrogen of the jet prominences is ejected with a velocity many times exceeding that which would be necessary

to carry matter altogether away from the Sun, if he were not surrounded by an atmosphere.* Now since other matter is ejected along with the hydrogen, and some of this other matter is certainly denser than hydrogen, under equal circumstances of pressure and temperature, we cannot but believe that such matter undergoes far less retardation than the hydrogen in passing through the solar atmosphere. *How much* less we cannot say; but it seems no rash assumption to suppose that the metallic vapours (*à fortiori* if they condensed into metallic globules) would retain a velocity sufficient to carry them even beyond the observed limits of the corona.

But if any evidence were really required to prove that the power of the solar eruptions is sufficient for our theory, we need only consider the circumstances of the eruption witnessed by Professor Young on September 7, 1871 (see pp. 320–325). For it will be noticed that he saw matter carried from a height of less than 100,000 miles to a height of more than 200,000 miles in ten minutes. Now I have gone through the requisite calculation for determining in what interval of time this distance would be traversed by a projectile *in vacuo*, having 200,000 miles as the upper limit of its flight; and I find that no less than twenty-five minutes

* We could scarcely expect to detect spectroscopic evidence of these enormous initial velocities—first, because it is probable that the real jets have a relatively minute cross-section; and, secondly, because by communicating motion (gradually diminishing with distance) to the surrounding photospheric and chromatospheric matter, the outflowing hydrogen tends to mask (by widening the lines) the spectroscopic evidence of its own motion.

fifty-six seconds would be required. Now, on the supposition that there was no retardation, we should have to suppose that when the hydrogen wisps became invisible (doubtless through cooling), they still continued to move upwards, and that they reached a height of about 350,000 miles before their upward motion ceased. For in this case the observed distance would be traversed in the observed time. Even this altogether incredible supposition forces us to believe in a velocity of ejection of about 255 miles per second. But if we assume (what is altogether more likely) that the hydrogen ceased to move upwards after reaching a height of about 200,000 miles, we must infer that it was projected with a much greater velocity than 255 miles per second. For this velocity would only carry it across the observed range in the observed time when not subject to any retardation within that range. But it was subjected in reality to such an enormous retardation that, instead of passing to a height of 350,000 miles,* it was brought to rest at a height of little more than 200,000 miles. Therefore when at a height of 100,000 miles (or at the beginning of its observed course) it must have been travelling much faster than a projectile, *in vacuo*, making for a height of 350,000 miles. So that even if projected to that height through a vacuum the uprushing hydrogen would have required a much greater initial velocity

* The whole reasoning is very much strengthened if we suppose that the real seat of eruption lay some distance from the Sun's limb,—or that the motion of the hydrogen was so seen as to be foreshortened.

than 255 miles per second. But so far from the retardation being *nil* in the lower half of the hydrogen's course, it must have enormously exceeded the retardation in the upper half (since the atmospheric pressure increases not only rapidly, but with constantly increasing rapidity, as the Sun's surface is approached). Thus it is not merely probable, but certain, that in the case of this outburst, the ejectional velocity exceeded enormously—exceeded *many times*—the utterly inconceivable velocity of 255 miles per second.

Furthermore, since the velocity due to a projectile *in vacuo*, flung from the Sun's surface to a height of 200,000 miles, amounts to but 212 miles per second, it is not only probable but certain, that in this instance the velocity thus estimated falls enormously short of the real velocity of ejection. Hence, in all jet prominences the like must be inferred. It may be concluded that the matter of the smallest jet prominence actually recognisable as such must have been ejected with a velocity sufficient, if there were no atmosphere, to carry the matter for ever away from the Sun.

Now when we remember what the spectroscope has taught us respecting the constitution of the corona, we cannot but see that the inference deducible from the preceding reasoning — the inference, namely, that erupted metallic vapours pass away to a far greater distance than the erupted hydrogen—is very much strengthened. It is noteworthy also that the polariscopic evidence has been regarded by Professor Stokes (the highest English authority on the sub-

ject) as seeming to imply the condensation of metallic vapour moving on radial lines from the Sun, into liquid or solid particles of exceeding minuteness.*

And here we are brought to consider several matters which bring our present subject into somewhat unexpected relationship with other departments of cosmical research. Space will only permit me to touch very briefly upon them.

If matter is thus propelled from the Sun so constantly as to account for the permanence of the corona, it can scarcely be doubted that, exceptionally perhaps, or perhaps not uncommonly, matter is projected as far as the Earth's orbit, or even beyond. One would even expect that no inconsiderable proportion of the Sun's substance might be ejected with such velocity as to pass for ever away from his dominion.

Now in the case first mentioned, it would be reasonable to anticipate that, as a direct result, the number of aerolitic masses falling on our earth by day would somewhat exceed the number falling at night. For if a flight of erupted globules (gathering probably into a single mass as they travelled onwards) came into direct contact with the Earth, that face of the Earth turned at the moment towards the Sun must necessarily be the scene of the encounter. Now, as a matter of fact, it has been observed that more aerolites fall near noon

* He conceives that their dimensions fall short of the wave-lengths of light; and he suggests magnesium as the element principally in question. It does not appear, however, that he lays stress (so far as optical considerations are concerned) on the nature of the elementary constitution of these metallic clouds.

than at any other hour. We have this on the authority of Humboldt, a careful student of the subject of meteoric astronomy, and the fact is confirmed by the systematic researches of Alex. Herschel, Heis, and others in our own time.

As to the second case, the point to be chiefly noticed is this: that if our Sun sends forth such missiles towards the domains of other suns, analogy should guide us to the conclusion that those other suns—the stars—must also, from time to time, eject metallic matter from their substance. If this is so, we might expect that some of the meteors which reach our Earth would afford some indications of their stellar origin.

For example, their velocity might be in excess of that due to the Sun's attracting influence (see p. 86); in which case no explanation would be so plausible as the very conclusion towards which we are now being forced. But here again, as a matter of fact, the most careful observers, Alexander Herschel in England, Professor Newton in America, Fr. Secchi in Rome, Heis in Germany, tell us of meteoric velocities of 60, 70, 80, even 90 and 100 miles per second. Such velocities, if real (and it is difficult to question many of the observations on which these results have been based), correspond exactly with the results to which we have been just led.

Again, the structure of meteors, if carefully examined, might be expected to reveal evidence of their having once existed within the interior of an orb like our Sun. But Mr. Sorby, so far back as 1864, wrote

thus respecting the microscopic examination of certain meteors:—'The most remote condition of which we have positive evidence, was that of small detached melted globules, the formation of which cannot be explained in a satisfactory manner, except by supposing that their constituents were originally in the state of vapour as they exist in the Sun.' He found evidence that the meteors had been in the state of vapour while under enormous pressure, and 'in mountain masses.'

Yet again, the chemical structure of meteors might be expected to supply evidence of a stellar origin. Now, the late Professor Graham, after a careful analysis of the Lenarto meteor, reasons thus: 'It has been found difficult to impregnate malleable iron with more than an equal volume of hydrogen under the pressure of our atmosphere.' Now, 'the meteoric iron (this Lenarto iron is remarkably pure and malleable) gave up almost three times that amount without being fully exhausted. The inference is, that *the meteorite had been extruded from a dense atmosphere of hydrogen gas*, for which we must look beyond the light cometary matter floating about within the limits of our own system. . . . . Hydrogen has been recognised in the spectrum analysis of the light of the fixed stars by Messrs. Huggins and Miller. The same gas constitutes, according to the researches of Fr. Secchi, the principal element of a numerous class of stars, of which *a* Lyræ is the type. *The iron of Lenarto has no doubt come from such an atmosphere in which hydrogen*

*greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, the hydrogen of the stars.'**

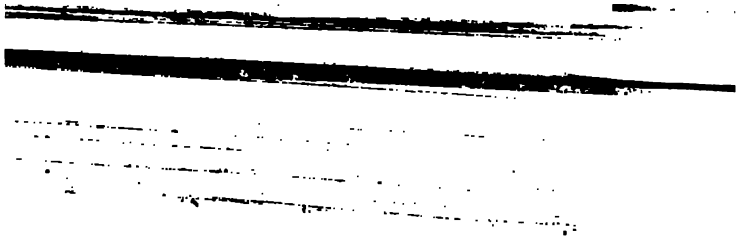
I would further invite the attention of the reader to the account of the behaviour of Sun-spots and faculæ (see pp. 214, 215); to Gilman's account of the aspect of a large prominence (p. 310), and to the reasoning on the striations observed by him (pp. 380, 381); and, lastly, to Zöllner's account of linear flashes in the spectrum (p. 310). All these matters bear very pertinently, as it seems to me, on the theory we are now considering; and they acquire fresh force from the fact, that they have not been introduced here for the purpose of supporting the theory, but appeared in the first edition of this work, in which the theory was not advocated.

Even the perplexing relation considered at p. 216, may find an explanation in the theory now under discussion. For regarding the spots as phenomena of eruption, that is, as beginning with eruption, we might expect that their occurrence would be associated with the planetary motions of recess or approach. Eruptions and earthquakes on our own Earth, stable as is its substance by comparison with the Sun's, occur more frequently when the Moon is in perigee; and Sir John Herschel has explained the predominance of active

* See further 'The Fuel of the Sun,' by Mr. W. Mattieu Williams, F.C.S., in which the theory of the expulsion of matter from the Sun is ably advocated. Some of his inferences are not in accordance with strict dynamical principles; but the book is well worth careful reading.

volcanic and earthquake regions along shore-lines, as depending on the seemingly insignificant changes due to tidal action. How much more, therefore, might we expect that the solar equilibrium would be disturbed by planetary action, when all that has been revealed respecting the Sun tends to show that the mightiest conceivable forces are always at work beneath his photosphere, one or other needing only (it may well be) the minutest assistance from without to gain a temporary mastery over its rivals. And if, as recent observations tend to show, the mightiest of the planets sympathises with solar action—if when the Sun is most disturbed, the belts of Jupiter are also subject (as of late, and in 1860) to strange phenomena of change—how readily do we find an explanation of what would otherwise seem so mysterious, when we remember that as Jupiter disturbs the mighty mass of the Sun, so the Sun would reciprocally disturb the mass of the largest of his attendant orbs.

The reasoning which has led to the suggestion that electrical processes are at work in producing a portion of the coronal light, as well as that which has seemed to many acute minds to support the theory of an atmosphere of extreme tenuity, extending millions of miles from the Sun, would appear to be confirmed by, and to confirm the arguments for, the emission theory. For electrical action could not be excited where the coronal streamers are seen, save by the rush of matter projected radially from the Sun through a medium resembling an atmosphere.



Furthermore, the meteoric theory advocated above —a theory quite distinct from that of a meteor-generating emission—need by no means be abandoned, if the emission theory is regarded as established. It will remain for future observations to determine what proportion of the coronal light is due to the several causes here considered; but it can scarcely be doubted that an appreciable proportion of that light is due to each.

The results of the eclipse expeditions of December 1871 were the last which added importantly to our knowledge respecting the corona. In the first place, seventeen excellent photographs, agreeing admirably in all essential respects, were obtained at Bekul, Dodabetta, Avenashi, and Jaffna. Plate IX. has been formed by combining together all the information given by these pictures. Next, the existence of Young's complex and relatively shallow atmosphere, giving a spectrum of multitudinous bright lines, was finally established. Thirdly, Respighi observed what Young had demonstrated, the fact, namely, that the gaseous matter of the inner corona extends upwards of 200,000 miles from the Sun's surface; for he saw three coloured spectral images of the corona with this degree of extension (two of these images being due to glowing hydrogen). Fourthly, Janssen proved that besides Kirchhoff's 1474-line, the hydrogen lines and several other fainter lines exist in the gaseous spectrum of the corona, while he has been able to detect the solar dark lines (notably the sodium line) across

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meteoric matter is propelled from our Sun and his fellow suns. These meteoric outbursts reveal the future to us as no less infinite than the past. We may truly say of time as thus pictured to us, what, in Goethe's wild vision, the Angel said of space:—'End is there none to the universe of God. Lo! also there is no beginning.'

with results which cannot but be regarded as trustworthy.

But when we attempt to combine these several results, and further to determine what the general condition of that orb may be which presents these several features, we recognise at once that a problem of enormous difficulty lies before us. The more we have learned respecting the Sun, nay the more we have learned respecting those physical laws by which we are to interpret solar phenomena, the more insuperable have our difficulties become. It was easy to theorize when as yet but little was known. It was easy to suppose that the few physical laws we imagined we understood sufficed to account for all the phenomena presented by the solar orb. But as one fact after another has been discovered, the true complexity of the problem has been revealed to us; and as the physical laws which it is in our power to discuss and experiment on have been more carefully studied, we have begun to recognise how very limited our experience has hitherto been. It is not too much to say that theories respecting the Sun's physical condition which would have been regarded twenty years since as deserving of careful study, have now no worthier standing in science than the idea of Anaximander that 'the Sun is a great vessel filled with fire, at the top of which is an opening through which the fire escapes.'

It is easier to consider those facts which have revealed to us the enormous difficulty of the problem we

are upon, than to present any considerations tending to render our conceptions clearer.

In the first place, we recognise the fact that in the Sun the elements exist in conditions altogether different from those we are familiar with. This is true of all orders of elements, from those whose normal condition (as recognised by us) is gaseous, to those which at ordinary or even very high temperatures remain solid or liquid. For instance, metals which we can volatilise indeed in small quantities, and by the aid of special contrivances, but which yet we can only experiment upon (properly speaking) when they are in the solid or liquid state, are present in the Sun as glowing vapours. But also gases which no amount of pressure or refrigeration we can command will cause even to show signs of approaching the liquid condition, probably exist in certain portions of the solar globe as liquids or even as solids. On the one hand, then, we have an inconceivably high temperature volatilising our most fixed elements—nay, for aught we know, perhaps dissociating substances which we regard as elements into their true primary constituents; on the other, we have an inconceivably high pressure at a relatively inconsiderable distance beneath the photosphere, reducing our so-called perfect gases into the liquid or solid form.*

* I do not know that we have any sufficient evidence that hydrogen, oxygen, nitrogen, and other gases which we call perfect, are really exceptions to the general rule that all substances are capable of assuming —under suitable conditions—the solid, liquid, and gaseous form. It is true that no pressure or refrigeration we can apply causes these gases

In the next place, we have learned of late to recognise on how very doubtful a basis many of the received axioms (almost) of physical science have been placed. No laws of science were perhaps more thoroughly accepted than those which were supposed to distinguish the solid, liquid, and gaseous states from each other. All physicists believed that a definite and well-marked change of condition necessarily accompanies the process by which gas passes into the liquid state, and very few were disposed to doubt that a correspondingly distinct line of demarcation separates the liquid from the solid state. Yet the researches of Dr. Andrews have recently shown that under certain conditions carbonic acid gas may be made to pass by absolutely insensible gradations from an undoubtedly gaseous to an undoubtedly liquid state; and it is recognised that what has been proved in the case of a single gas is in all

to exhibit the least trace of those qualities which the imperfect gases exhibit at ordinary temperatures and pressures. In other words, the perfect gases, as their title implies, obey perfectly (1) the law associating density and pressure; and (2) the law according to which the relation of specific heats at constant pressure and constant volume remains constant. But it is known that imperfect gases at ordinary temperatures and pressures seem very closely to obey these laws, though as they approach more and more nearly to the circumstances under which they become liquid (whether through increased pressure, simple refrigeration, or both combined) they depart in a marked way from these laws. And it is probable that if the imperfect gases could be experimented on at enormously high temperatures and low pressures, they would be found to obey the above-named laws as perfectly to all appearance as the perfect gases do. All we can assert respecting the perfect gases is that none of the processes of combined pressure and refrigeration hitherto applied —perhaps none we are capable of applying— bring them even so far towards the state under which they would become liquid as the imperfect gases are brought under ordinary pressures and temperatures.

probability true of all gases and vapours. It has been also rendered highly probable that under suitably great pressure the passage of a molten metal to the solid form, or conversely the melting of such a metal, may take place in a gradual manner, so that at a certain stage of the process it shall be impossible to say whether the metal or certain portions of it be liquid or solid.

Another physical law had been thought to be thoroughly established. It had been supposed that the whiteness (or true incandescence) of flame was due to the presence of minute particles of incandescent matter. But Frankland has shown that with increase of pressure the faintly luminous light of burning hydrogen may be rendered bright, and that by sufficiently increasing the pressure the spectrum of the light becomes continuous. So that what had been supposed the most marked characteristic of incandescent solid and liquid bodies, is thus shown to be a possible characteristic of the light of glowing gas. Thus the whole basis of our reasoning (which had been thought so sound) respecting the actual condition of the solar photosphere, whether as evidenced by direct observation or by analysis with the spectroscope, has been shaken.

Yet again we have learned to recognise the fact that the imagined limits to the rarefaction at which gases may subsist, as such, had been placed far too low. Our experimenters have gone indeed very far towards the production of an actual vacuum, without obtaining any evidence that the almost infinitesimal

quantity of gas which can alone remain in the so-called vacuum-tubes, behaves otherwise than at appreciable pressures.*

It will be seen at once how importantly this bears on the subject of solar physics, since it compels us to reconsider all our ideas respecting the probable limits of atmospheric envelopes; and the most difficult questions of solar physics are precisely those which depend on this matter. Unfortunately the observed fact gives us no new means at present of forming a satisfactory opinion as to atmospheric limits. Even if it should be regarded as demonstrating that there is no real limit to atmospheric extension, we should still be no nearer than before to a determination of the atmospheric pressures at the surface of the Sun or of any planet

* Physicists have carried the rarefaction so far that the electric spark will no longer traverse the tenuous medium; and some have been led to suppose that when this state of things has been brought about there must be an actual vacuum. No such conclusion can be regarded, however, I will not say as demonstrated, but as probable or even conceivable. All the evidence we have tends to show that an absolute vacuum is as imaginary a conception as the philosopher's stone or the perpetual motion. Take the most stable and dense metal, platinum, and conceive that for a moment in the heart of a mass of such metal there was a vacuous space; then in an instant that space would be occupied. For, even setting aside the probability that the most solid metals undergo an indefinitely minute but still real dissipation at their surfaces, corresponding to that dissipation which ice undergoes at the lowest temperatures yet experimented on—setting aside, I say, this probability, there yet remains the certainty that in the intimate molecular structure of the platinum a perfectly free communication exists between the imagined space within and the space without. The communication is not, indeed, such as suffices for the conveyance of certain forms of matter or of motion; but that it is none the less real on that account cannot be questioned.

except our own Earth; and this is the problem which chiefly concerns us here.*

Still further, in considering the Sun's physical condition, we have to discuss the effect of motions wholly surpassing in velocity any that we are familiar with on Earth, and therefore *à fortiori* any that we can experiment upon. Setting aside the fact that matter from without must continually be falling upon the Sun (in whatever condition) with velocities such as have been dealt with in Chapter II., we have the observed fact that movements fairly comparable with these have been recognised in the very substance of the Sun by the aid of spectroscopic analysis. Now, what means have we for determining the probable effect of motions of 100 miles per second taking place even among substances in conditions such as we are familiar with? We know the effects of certain velocities; we see the bullet melted as it reaches the target, the meteor vaporised as it speeds through the air. But we have no means whatever of determining what effects would be produced by velocities enormously exceeding even the inconceivable velocity of many meteors. When to this we add that the swift motions referred to take place

* It would, indeed, be pleasant to theorise on this matter, to conclude, for example, that every planet could have just so much atmosphere as corresponded to the range of its attractive influences; and consequently to deduce the atmospheric pressure upon the Sun. But no confidence could be placed in such theories. For, on the implied supposition, quite other forces than attraction would have to be considered. And, further, it would be an inevitable consequence of such a state of things that as the planets changed their relative positions, the atmospheric pressure of the surface of each would vary.

amid depths of vaporised metals, and, for aught we know, over seas and continents of liquefied and solidified gases, we may well shrink from the task of attempting—at least in the present state of our knowledge—to estimate their probable effects.

I shall limit myself, therefore, to an exceedingly brief statement of certain general relations, which are all that appear satisfactorily established by what has as yet been learned respecting the Sun.

We have in the Sun an agglomeration of the elements we are familiar with on the Earth; and this vast agglomeration is subject to two giant influences, producing in some sort opposing effects—viz., a temperature far surpassing any we can form any conceptions of, and a pressure (throughout nearly the whole extent of the solar globe) which is perhaps even more disproportionate to the phenomena of our experience. Each known element would (beyond all question) be vaporised by the solar temperature at known pressures; each would (there can be little question) be solidified by the vast solar pressures, did these occur at known temperatures. Now, whether under these circumstances the laws of gaseous diffusion prevail where the elements *are* gaseous in the solar globe; whether where liquid matter exists it is in general bounded in a definite manner from the neighbouring gaseous matter; whether any elements at all are solid; and if so, under what conditions their solidity is maintained and the limits of the solid matter defined—all these questions are such as we *must* answer before we can form a

satisfactory view of the solar constitution; and yet they are questions which we have at present no means of answering. Again, we *must* learn how far combustion, properly so called, can take place within the Sun's mass, and whether those processes which we recognise as combustion are the only processes of combustion which can actually take place there. For aught that is yet known, the intensity of the forces at work upon and within the Sun may wholly prevent the occurrence of any processes of combustion familiar to ourselves; while other processes of true combustion altogether unthought of by us may be in continual action.

Assuming, however, that some general resemblance exists between the processes at work upon the Sun and those we are acquainted with (the wildest assumption possible), we may imagine that the various elements in the solar substance ordinarily exist down to certain definite levels in the gaseous form, at lower levels (definite for each) in the liquid form, at yet lower levels in the solid form. That part of each element which is gaseous must again be divided into two portions—that whose light is capable of giving characteristic spectra of lines or bands (these spectra being, however, different for portions lying at different depths), and that lower portion whose light is capable of giving a continuous spectrum.

Now, here we approach the great difficulty of interpreting the results of the spectroscopic analysis of the Sun. We have no means of learning whence that

part of the light comes which gives the continuous spectrum. When we recognise certain dark lines, we know certainly that the corresponding element exists in the gaseous form at a lower temperature than the substance which gives the continuous spectrum. And so, also, we can interpret the appearance of bright lines. They show beyond question that the corresponding element exists in the gaseous form at a higher temperature than the substance which gives the continuous spectrum. But as regards that continuous spectrum itself we can form no such exact opinion. It must be remembered that a substance giving a continuous spectrum is not necessarily opaque to light from a substance at higher temperature also giving a continuous spectrum. It is capable of exercising a general absorption, but not necessarily (nor probably, under such conditions as exist in the Sun) of exercising an absorption at once general and complete. Hence we have no means of determining how great a depth of the solar substance is concerned in sending out the light which gives the continuous background of the spectrum. This light may come from the surface layers only—but it may be a shell whose thickness forms no inconsiderable aliquot part of the Sun's diameter.* And the reversal of the lines of certain

* This is in no way opposed to the evidence adduced by Sir John Herschel from the apparent uniformity of light derived from a glowing and transparent liquid not uniformly deep; for every part of such a liquid (at least in Sir J. Herschel's experiment) is at appreciably the same temperature. If the lower layers could be heated to a higher temperature, the effect of depth would become apparent.

elements, although it cannot take place at such excessive depths, may yet take place very far below the visible limits of the photosphere. For, as I have already shown, a depth of a few hundred miles would be wholly inappreciable in the most powerful telescope (spectroscopically armed), and so no peculiarities would be recognised as the result of processes taking place within such distances of the solar surface, however diligently the edge of the solar disc might be examined with the spectroscope. And further, as respects the examination of the Sun's edge, on which so much stress has been laid, it is far from unlikely—if, indeed, it is not to be regarded as certain—that the visible edge of the solar disc lies considerably above the true limit of the photosphere. That light at the Sun's edge which seems to belong to the hemisphere of photospheric matter turned towards us comes probably from parts of the photosphere which lie really beyond the borders of that hemisphere, and are simply brought into view through the refractive power of the lower layers of the solar atmosphere. I am not here venturing a mere opinion or conjecture—though I profess no certainty of conviction in the matter. We have the evidence of facts—observed by Carrington, Secchi, and others—showing that the motions of the spots across the solar disc are really affected, as respects apparent *rate*, by the refractive power of the solar atmosphere. And it is impossible to doubt that if the apparent place of a spot can be affected in this

way, then the solar regions beyond that hemisphere which is turned towards us at the moment, must be brought into view and form the real limits of the solar disc.*

I am sensible that I am not making definite statements as to the Sun's condition, but only stating difficulties. The difficulties are real, however, not imaginary, and ignoring them can serve no useful purpose.

When we turn to the details of the solar orb and its surroundings, we find the evidence slightly more definite; but still greater difficulties surround us. In the course of the several chapters bearing on these matters, nearly all the known facts which bear directly on the views we are to form respecting the Sun's physical constitution have been discussed as they have been described, so that but few words about the several solar features are here called for.

Taking the actual telescopic aspect of the solar spots and their surroundings, a perplexing series of problems is suggested. These problems are indeed so perplexing as abundantly to justify the disagreement hitherto found among theorisers. Admitting the spots to be depressions, what is the real disposition of the matter

* This really amounts to saying that the Earth, if viewed from the solar photosphere, would be visible above the solar horizon (as our Sun is after real sunset and before real sunrise) when the geometrical line to her passed below that horizon. We cannot doubt that the solar atmosphere would exert this refractive effect at least as powerfully as our own; and if the Earth could be seen in this way from parts of the Sun really turned away from her, then certainly those parts of the Sun must be visible from the Earth; for a visual ray passes along the same path from whichever of its extremities we suppose it to travel.

which produces the appearances we call faculæ—pen
umbra—umbra—nucleus? The penumbra may be
long to a lower layer, but the general arrangement
according to which the willow-leaves on the penumbra
point inwards towards the umbra seems to indicate a
real connection between the penumbra and the facu
lous bordering. This arrangement is indeed sometime
so marked that one is led to imagine that the so-called
willow-leaves are filamentous bodies which usually
hang in a nearly vertical position and so appear nearly
round, but when thrust aside during the formation o
a spot hang nearly horizontally, the ends which ha
been lowest floating like streamers towards the regio
whence they had been removed. If we could but con
ceal from ourselves a large portion of the evidence w
have (or else explain it away) this view might be in
sisted upon with pleasing confidence; but as a matte
of fact it merely serves to indicate the impression pro
duced by certain phenomena, and has at present n
value whatever.*

A great difficulty lies in the fact that we have n
clear evidence to show whether the Sun-spots ar
formed by forces acting from without or from within
Here I set on one side the theory that in a spot w
see a region where a great heat has dissolved solid c

* Lest I should here be supposed to be too curtly criticising the view
of others, let me hasten to say that the fancy thus summarily rejected
my own, and, so far as I know, as original as it is probably valueles
Yet I have not introduced it without a purpose. There is at least
much evidence in its favour as in favour of many theories which ha
been very confidently put forward.

liquid or cloudlike matter forming the photosphere, and that thus the intensely hot, but feebly radiating gaseous nucleus of the Sun (according to this theory) is disclosed. Kirchhoff has fairly disposed of this theory by showing that this intensely hot nucleus would be transparent to the light from the farther side of the Sun, and that therefore no spot could appear unless two openings on opposite sides of the Sun happened to be in the same visual direction.* I refer now, not to this or similar theories, but to the definite problem, whether the seat of that action which leads to the formation of a spot lies below or above the level of the photosphere. The spectroscope shows that a spot is a region where certain gases exist at a lower

* Fr. Secchi was the original propounder of this theory (not M. Faye, to whom it is usually ascribed). The theory really does account for many observed features of the solar spots, but it is none the less untenable. Fr. Secchi's answer to Kirchhoff's objection would seem to indicate that he has not recognised the exact force of that objection. He says it is not true that a gaseous nucleus would be perfectly transparent to rays from the farther side of the Sun, for we see that our own atmosphere absorbs light as well as heat. Kirchhoff's argument is that the solar nucleus would be transparent on account of its existing at a higher temperature than the photosphere—according to the theory at least which he deals with.

Since the above was written, Fr. Secchi has made the theory the subject of a reclamation, and Faye has answered that he claims only the portion of the theory relating to the motion of matter within the solar orb. On this last subject his reasoning is somewhat remarkable. He considers that the existence of currents within the solar orb, whereby various portions of his gaseous substance are successively brought to the surface, would account for the continued emission of light and heat; whereas a more stable condition would result in the cessation of radiation in the course of a few days. Surely the precise reverse of this is the case, and the loss of heat would be more rapid in the former case than in the latter.

temperature than in other parts of the Sun. But whether this low temperature results from the expansion of compressed gas erupted from the Sun, or from the fact that matter has reached the Sun from outer space, remains as yet undetermined. The evidence recently obtained respecting the eruption prominences seems, however, to favour the former view.

Again, as to the prominences, it seems to be demonstrated that some are mere clouds in the upper region of the solar atmosphere, while others are due to some form of eruption, and only assume the cloud form after the eruption which gave them birth has ceased. But what are the circumstances which give birth to these eruptions, what the nature of the layer (Zöllner's 'Trennungschicht') beneath which the eruptive action is prepared, and what the actual depth whence the erupted matter springs, we have very little to show.

And lastly, as to the corona and the general relations involved in the access of external matter from the interplanetary and intersidereal spaces to the neighbourhood of the Sun's globe, and the emission of matter from within that globe, we have, I apprehend, small means of forming definite opinions. The condition, indeed, of the space which lies immediately around the Sun is still very little understood by us. It may be that in the study of the corona during total eclipses we may find a means of answering the many perplexing questions associated with this matter. It may even be that new appliances may enable us to study the corona when the Sun is not eclipsed, and so

to learn whether systematic processes affecting the Sun's economy are at work in the region immediately surrounding him. At present our information on this subject is meagre in the extreme; and our means for acquiring information are far from promising. Here, as in so many matters relating to the physical constitution of the Sun, we must perforce wait until our experimental knowledge and our instrumental means have been very largely increased.

CHAPTER VIII.

THE SUN OUR FIRE, LIGHT, AND LIFE.

FEW of the results of modern scientific research are more remarkable than the recognition of the real extent of the influence which the Sun exerts upon the Earth. Of old the Sun's power as ruler over the seasons, his action upon vegetation, and other like influences, were recognised in a vague and general way. But men were far from regarding the Sun as the true source of many forms of force which seem almost equally important. Still less were they prepared to trace his influence in nearly every kind of action or mode of motion taking place upon our globe. It is the most striking feature of recent scientific research that it has taught us to see in nearly all terrestrial phenomena the action of a certain proportion of Sun-force.

We owe to the greatest astronomer of our time—Sir John Herschel—the first definite enunciation of this great principle. 'The Sun's rays,' he wrote in 1833, 'are the ultimate source of almost every motion which takes place on the surface of the Earth. By its heat are produced all winds, and those disturbances in

the electric equilibrium of the atmosphere which give rise to the phenomena of lightning, and probably also to terrestrial action and the aurora. By their vivifying action vegetables are enabled to draw support from inorganic matter, and become in their turn the support of animals and man, and the source of those great deposits of dynamical efficiency which are laid up for human use in our coal strata.* By them the waters of the sea are made to circulate in vapour through the air, and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of the elements of nature, which by a series of compositions and decompositions give rise to new products, and originate a transfer of materials. Even the slow degradation of the solid constituents of the surface, in which its chief geological change consists, is almost entirely due—on the one hand to the abrasion of wind or rain and the alternation of heat and frost, on the other to the continual beating of sea-waves agitated by winds, the results of solar radiation. Tidal action (itself partly due to the Sun's agency) exercises here a comparatively slight influence. The effect of oceanic currents (mainly originating in that influence), though slight in abrasion, is powerful in diffusing and transporting the matter abraded; and when we consider the immense transfer of matter so produced, the increase of pressure over large spaces in

* This remark is commonly but erroneously attributed to the celebrated engineer Stephenson. It appears in the first edition (1833) of Herschel's *Outlines of Astronomy*.

the bed of the ocean, and the diminution over corresponding portions of the land, we are not at a loss to perceive how the elastic force of subterranean fires, thus repressed on the one hand and released on the other, may break forth in points where the resistance is barely adequate to their retention, and thus bring the phenomena of even volcanic activity under the general law of solar influence.'

Since this was written men of science have learned to enounce the complete law of the 'conservation of solar energy,' as applied to the organic and inorganic world. That which was put forward in a general way by Sir John Herschel has been made the subject of special scrutiny. We have learned how to weigh and measure the Sun's action and the force-supplies which we derive from it.

Let us take first the supply of heat the Earth derives from the Sun. We shall have much to excite our wonder, whether we regard the real vastness or the relative minuteness of this supply.

From the researches of Sir John Herschel it appears that the direct heat of the Sun, if 'received on a surface capable of absorbing it and retaining it, would suffice to melt an inch of ice in thickness in 2h. 13m.;' and he thence calculates that no less than 26,000 tons of ice would be melted per hour by the heat actually thrown on a square mile exposed at noon under the equator. This amount must be multiplied fifty million times to correspond to the heat actually received by the Earth's globe during a single hour. Pouillet

obtained results not differing very greatly from these. He calculated that an interval somewhat greater than 2h. 13m. would be required to melt a layer of ice one inch thick.* Expressing his result according to a somewhat different method, he states, again, that if the Sun's heat were distributed uniformly over the Earth's surface 'it would in one year suffice to liquefy a layer of ice 100 feet thick, or to heat an ocean of fresh water sixty miles deep from the temperature of melting ice to the boiling point.'

Yet this enormous annual supply of heat is but the 1-2,138,000,000th† part of that which the Sun actually radiates into space in the course of a year. All the planets of the solar system are able to intercept but about the 227-millionth part of the heat actually emitted by the Sun. There is a fine passage in Herschel's 'Outlines of Astronomy' which shows how enormous is the amount of heat deduced by increasing in the ratio above indicated the supply of heat actually received from the Sun by the Earth:—'Supposing a

* The actual relation between Pouillet's and Herschel's results may be thus expressed. Sir John Herschel deduced 43·39 feet as the thickness of ice which the Sun is capable of melting per minute, supposing the ice continually applied to the Sun's surface (and the water produced by its fusion continually carried off). Pouillet deduced 38·7 feet per minute. Sir John Herschel says that 40 feet may be regarded as a probable mean. (It will be noticed that, with characteristic modesty and generosity, he places the mean much nearer to Pouillet's value than to his own.)

† It is singular how persistently the number 2,300,000,000, calculated by Mayer, maintains its ground in scientific treatises. This number was correctly deduced from the old value of the Sun's distance. But the above is the true value, according to the best modern estimates of the solar parallax.

cylinder of ice forty-five miles in diameter to be continually darted into the Sun *with the velocity of light*, the heat now given off constantly by radiation would then be wholly expended in its liquefaction on the one hand, while on the other the actual temperature at the Sun's surface would undergo no diminution.'

The luminosity of the Sun's surface is more readily estimated than the heat, since the intrinsic brilliancy of a self-luminous substance is in no way affected by distance; and we have only to take into account the effect which our own atmosphere may have in diminishing the apparent brightness of the Sun in order to form an accurate estimate of the intrinsic brilliancy of the Sun's light. Comparisons have been instituted directly between the light of the Sun and that of known terrestrial lights. It has been found that the most intense light we can produce appears absolutely black by comparison with the brightness of the solar orb. It has been estimated that the intrinsic brilliancy of the Sun's surface exceeds more than 146 times the brilliancy of the lime-light, and 32,700 times that of a sperm candle. In order to conceive the real amount of light to which a body close by the Sun—within a foot, say, of the photosphere—would be exposed, we must conceive the amount of light we receive in the full splendour of a summer's day increased in the same proportion that the whole hemisphere of sky exceeds the solar disc—besides, of course, a further addition corresponding to the proportion in which the brilliancy of the solar disc, if viewed without the interposition of

any atmosphere, would exceed the actual brilliancy observed on a summer day.

Of the chemical activity of the solar rays, it is not in our power to speak with so much confidence, since we have not as yet measured the Sun's power in this respect in a way which enables us to pronounce on its real extent. We can compare the intensity of the Sun's chemical action with that of terrestrial lights; but we have not yet found a means of determining its value as compared with those forces which the chemist more ordinarily employs to produce chemical changes.

It is worthy of notice, as respects the last two forms of solar activity, how large a share of the force we derive from the Sun is obtained through their action. This will be apparent when we remember the important bearing of the processes of vegetation on the wants of the human race. 'Nature,' says Mayer, 'has proposed to herself the task of storing up the light which streams earthward from the Sun—of converting the most volatile of all powers into a rigid form, and thus preserving it for her purposes. To this end she has overspread the Earth with organisms, which, living, take into them the solar light, and by the consumption of its energy, incessantly generate chemical forces. These organisms are *plants. The vegetable world constitutes the reservoir in which the fugitive solar rays are fixed, suitably deposited, and rendered ready for useful application. With this process the existence of the human race is inseparably connected.*'

And even if we regard the effect of the Sun's heat as exerted upon the oceans and continents of our globe, we find that a large proportion of that which is eventually utilised by man, in one way or another, is first made subservient to the processes of vegetation. When the Sun's rays are poured down upon the ocean, or on parts of the Earth's surface in which water is abundant, the heat raises into the atmosphere large quantities of aqueous vapour. And this vapour, rising by reason of its extreme lightness, reaches eventually a region where it is condensed into *clouds*. Again, the heat of the Sun producing various effects, according to the nature of the regions on which it falls, gives rise to those differences of temperature which result in the generation of *winds*. By the agency of winds the clouds are transferred from the place of their formation to regions which require to be nourished by copious showers. And thus winds and clouds combine to support vegetation. The winds convey the clouds from place to place, and the clouds themselves, in the expressive language of Scripture, 'drop fatness on the earth.'

It is worthy of notice, too, that beside the action of the Sun in supporting vegetation at the present time, it was the same form of action exerted in long-past ages which resulted in storing up for our use those vast supplies of energy which are contained within our coal-mines. In other words, what may be called our *force-principal* is as fully due to the Sun's action (direct or indirect) in promoting vegetation as

that *force-interest* which we derive each year from the Sun's seasonal action.

And here I may be permitted to dwell on considerations which, though bearing rather on the economy of our Earth than on the general subject of solar physics, yet illustrate in a significant manner the work which the Sun has been appointed to do. I may premise, indeed, that we have no means of determining what the Sun's influence on the other planets may be, however clear it may appear to us that we are not the only, nor even the chief, recipients of those stores of force he lavishes so abundantly. It is on this account that while I give to this treatise a title indicating the Sun's position in the solar system, I deal only in this chapter—the sole one bearing on the Sun's office—with his position as *our* fire, light, and life. If in the considerations I am about to urge the Earth only seems concerned, it is none the less probable that results affecting the economy of the whole planetary scheme are in truth illustrated.

We are accustomed to look upon the Earth as an inexhaustible storehouse whence all our wants may be supplied. Year after year we till the soil, and still there is no lack in the growth of all the vegetable productions needed by man; nor do our flocks and herds diminish, notwithstanding the enormous supplies of flesh-meat we are continually consuming. Taking the whole Earth, it is probable that the yearly produce of agricultural and pastoral labours increases at even a higher rate than that at which the human race is

increasing, so that were man content, as in old times, to draw upon the Earth's stores for the supply of his ordinary wants, there would be little fear of that store being ever exhausted.

But of late a change has passed over the aspect of the world. On every side a multitude of new inventions, and with them a multitude of new wants, are making their appearance. The stores which had been garnered up during long past ages of the Earth's history are being consumed with a rapidity which has already begun to alarm our men of science. It is true, indeed, that there is as yet little room for fearing that the terrestrial storehouse will soon be cleared of its contents. Even if the coal-mines of the world should be exhausted, there are still other force supplies; and doubtless the present rate of consumption might be continued, or even an increased rate maintained, for a period which seems indefinitely long when compared with the short span of life allotted to man.

But, after all, what are a thousand years, or even several thousand years, when viewed with reference to the history of the globe on which we live? If it could be shown that within two or three thousand years man will have exhausted all the stores of force which exist within the Earth, it surely might be urged with fairness that the present rate of consumption is unduly—selfishly great; that the wants of future races should be considered, and that a check should be put upon those processes of over-rapid advance on which we are in the habit of priding ourselves. Precisely as we

should hold it to be blameworthy that a rich man should use, merely for purposes of luxury or convenience, that which could be shown to be absolutely essential to the existence of a large number of his fellow-men, so it might fairly be held to be wrong for the present inhabitants of the Earth to exhaust, in contrivances intended to add to the luxuries or conveniences of life, those stores which are absolutely necessary to the well-being of future races.

In dealing, for example, with the question of terrestrial coal supplies, it will not suffice to point out that for a thousand or several thousand years they may be drawn upon as at present, or even more largely, without exhaustion. The thousand or thousands of years will pass as surely as those which have already passed, and the wants entailed by our wastefulness will be felt none the less, that for so many years there had been no failure in the supplies contained within the great terrestrial storehouse. What must be done, then, is to show that by the progress of that very course of events which results in the rapid use of those stores, the means will spring into existence for obtaining fresh and inexhaustible supplies. This is no idly speculative view, but the plain and obvious duty of the scientific world. Precisely as the superiority of civilised races over barbarous tribes is shown in nothing more clearly than in the fact that the former are not content, as the latter are, merely to supply the wants of the moment, or of a few days, but seek to make provision, not only for future years, but for the wants

of their immediate descendants, so it behoves the leaders of the great movement which during the last few years has so greatly changed the aspect of the human race, to show the superiority of the new order of things by a careful provision for, and anticipation of, the wants of the races which will inhabit the Earth thousands of years hence.

Without discussing the various forms of work which are being done upon the Earth, or considering the various agents employed in producing the motive power by which those forms of work are set in action, it may be simply stated that at present nearly all our motive force is obtained from *stored* Sun-force. It would be difficult to point to a single work accomplished by the aid of modern scientific appliances which has not resulted in exhausting to a greater or less degree the force which the Earth has been garnering up in long past ages for our use. It is in this all-important respect that the more modern forms of machine-work differ from other forms of work. I refer, of course, to machines driven by inanimate motive powers, and not to those worked by the direct action of animal force. The machine draws upon the Earth's *garnered* stores,* while the living worker draws upon the Earth's *periodical supplies* of force. In the former case, that is being used up which cannot be replaced; in the latter, what is consumed will be

* Those appliances in which advantage is taken of the action of the wind, rainfall (rivers), tidal action, and a few other natural processes, are to be excepted. Modern invention, however, is but seldom directed to the utilisation of these old-fashioned force-supplies.

restored in the ordinary course of nature. In one case it is our 'force-principal,' in the other it is our 'force-income' we are consuming. The distinction is all-important.

This is not the place to enter into a discussion of the methods by which the great problem—a problem not requiring immediate solution, but which in the long run will surpass all others in interest and importance—is to be solved. But I may indicate what is, I take it, the direction in which a solution will be found. We are now utilising what Professor Tyndall calls the Sun of the Carboniferous Epoch: our descendants will have to employ the Sun of their own epoch. The heat of the solar rays—mayhap also their light and their actinic energy—must one day be applied to work our machinery. Already men have felt the advantage of thus employing solar energy. They have not, indeed, as yet applied the direct action of the Sun systematically to their purposes.* But in an indirect manner they have utilised solar energy. The ships which sail upon our seas, the mills which are turned by water or by wind—these and many other devices of man have been contrived to utilise a portion of the Sun's heat. But the proportion thus utilised is almost indefinitely small by comparison with that which is actually available. It is only necessary to

* Ericsson has constructed a machine in which the solar rays supply the primary motive force. It has not, however, yet been demonstrated (though I have not the least doubt it will be at some future epoch) that the solar heat can be employed in a profitable—that is, a 'mechanically advantageous,' manner.

translate some of the ordinary phenomena of nature into the language of the familiar forces in order to see that this is so. For instance, the amount of energy involved in the production of rain is startlingly great when compared with our ordinary estimates of force. I have calculated that the force expended in the production of a day's steady rain over an area equal to that of the county of Middlesex would be equivalent to a mechanical power competent to raise 1,000,000,000 tons to a height of three miles!

Professor Tyndall has put in a striking form the relation which exists between the simpler processes of nature and those effects which seem to us the most apt exponents of power. 'I have seen,' he says, 'the wild stone-avalanches of the Alps, which smoke and thunder down the declivities with a vehemence almost sufficient to stun the observer. I have also seen snow-flakes descending so softly as not to hurt the fragile spangles of which they were composed; yet to produce from aqueous vapour a quantity which a child could carry of that tender material, demands an exertion of energy competent to gather up the shattered blocks of the largest stone-avalanches I have ever seen, and pitch them to twice the height from which they fell.'

And when we have thus seen what a tremendous amount of energy is involved in such processes as the formation of snow or rain in comparatively small quantities, we begin to recognise, though we are far from being able to conceive, how enormous is the potential energy which supplies the rainfall of the whole Earth.

We must remember, too, that a large amount of rain falls where it is not wanted, and that the energy of the Sun expended in the production of wind is in large part wasted. Clouds are raised by evaporation from the sea surface to fall on another part of the self-same waters. Storms are roused which blow with vehemence for a while, and then sink into rest without having accomplished any purpose necessary to the wants of terrestrial races. Here at once we see a large amount of energy not fully utilised. I do not indeed say that this apparently useless expenditure of force has no purpose in the economy of nature. Doubtless every natural event has its end and object. What I would dwell upon is, that if the energy which thus seems wasted could be made available to subserve human wants, it might be used without any fear that the economy of nature would suffer from such an application of her energies. And if this is true of the application of the indirect effects of solar energy, it is *à fortiori* true of the utilisation of the Sun's direct action —that is, of those solar rays to which the winds and the rains are due.

Now, if we assume that with the progress of science the power of thus employing to the full—or much more fully than at present—the forces which the Sun really expends upon the Earth will be acquired by man, we recognise the probability that science viewed generally is one of the means by which the efficiency of the solar energies is enlarged and extended. What is true of our Earth may be regarded as in all proba-

bility true of other worlds than ours. As on our Earth so probably in other worlds there are or have been eras during which the beneficent power given to our great luminary is used without any consciousness of its value. There are or have been more advanced eras when the return of day and night, the progress of the seasons, the nourishment of the lately sown seed by spring rains, and the whitening of the fields into harvest under the summer Sun, are watched with anxious interest. Then later follow the eras when the annual supply of truly vital energies seems to become insufficient, and when garnered stores of force are utilised by the thoughtful or ransacked by the too eager. And lastly, it may well be that in other worlds, as one day doubtless on our Earth, there will be eras when a more advanced degree of science will enable intelligent beings to derive from direct solar action the means of obtaining even larger supplies of force than they had been, able to gather from the Sun-work of past epochs garnered by nature for their benefit.

The ideas of the Sun's true position in the solar system thus suggested lend an enhanced interest to the question whence the Sun himself derives and recruits his energies. As Tyndall has finely written, 'How is the perennial loss made good? We are apt to overlook the wonderful in the common. Possibly to many of us—and even to some of the most enlightened among us—the Sun appears as a fire, differing from our terrestrial fires only in the magnitude and intensity of its

combustion. But what is the burning matter which can thus maintain itself? All that we know of cosmical phenomena declares our brotherhood with the Sun—affirms that the same constituents enter into the composition of his mass as those already known to chemistry. But no earthly substance with which we are acquainted—no substance which the fall of meteors has landed on the Earth—would be at all competent to maintain the Sun's combustion. The chemical energy of such substances would be too weak, and their dissipation too speedy. Were the Sun a block of burning coal, and were it supplied with oxygen sufficient for the observed emission, it would be utterly consumed in 5,000 years. On the other hand, to imagine it a body originally endowed with a store of heat—a hot globe now cooling—necessitates the ascription to it of qualities wholly different from those possessed by terrestrial matter. If we knew the specific heat of the Sun* we could calculate its rate of cooling. Assuming the specific heat to be the same as that of water—the terrestrial substance which possesses the highest specific heat—then, at its present rate of

* The absolute quantity of heat necessary to raise the average temperature of the Sun by any given amount—say one degree, and therefore the absolute quantity of heat which corresponds to the loss of say one degree of temperature from the average temperature of the Sun—will depend on the physical constitution of the Sun. To say merely that the Sun's substance subsists at such and such a temperature, is by no means sufficient to indicate the amount of heat which the Sun is capable of imparting. A mass of iron, for example, may be at a temperature precisely equalling that of an equal mass of boiling water; yet the boiling water will give out far more heat while passing to any given lower temperature than the heated iron will.

emission, the entire mass of the Sun would cool down 15,000 degrees in 5,000 years. In short, if the Sun be formed of matter like our own, some means must exist of restoring to it its wasted power.'

We have not as yet the means of satisfactorily answering the question thus suggested. Answers have been suggested, but no answer has yet seemed so satisfactory that men could regard the problem as disposed of. Indeed, 'the facts are so extraordinary,' as Tyndall has said, 'that the soberest hypothesis must appear wild.' Whether we conceive, with Mayer and Thomson, that the Sun's heat is maintained by the incessant downfall of cosmical bodies gathered out of space by the Sun's mighty attractive energies; or whether we follow Helmholtz in supposing that the gradual contraction of the solar orb is the mainspring of the solar energies; or whether we believe, with Secchi, that the dissociation of compound bodies in the Sun's substance is a fund of force to be gradually exhausted only as the dissociated elements unite in chemical combinations; or, lastly, whether we prefer the idea thrown out by Sir John Herschel that mayhap the vital energies of monstrous creatures—the willow-leaves of Nasmyth—are the true source of the great luminary's might, we have not overpassed by a step the amazing field of conjecture appertaining to our subject. Startling as these theories appear, they are not a whit more startling than the known facts which they are intended to interpret.

I have no wish to enter here into a detailed con-

sideration of any of the theories above referred to; but I wish to make a few remarks respecting those of Helmholtz and Thomson.

When we consider the evidence forced upon us by the present condition of the solar system, and by the nature of the observed motions taking place within it, we find it difficult not to believe that two great processes have for uncounted ages been at work within its limits. On the one hand, the evidence is very strong in favour of the view that a process of contraction from a nebulous condition has taken place not only in the case of the Sun, but in that of the planets and other members of the solar system. On the other hand, the evidence is absolutely demonstrative that at the present time uncounted millions of minute cosmical bodies are streaming in upon the Sun. We have, then, clear evidence that at least some portion of the Sun's energy is derived from each of the two sources now dealt with. We can hardly conceive that the process of solar contraction has come to an end; and certainly we have no proof in the apparent constancy of the Sun's volume that the process has ceased, since Helmholtz has shown 'that the shrinking of the Sun's diameter by one-10,000th part of its present length would generate an amount of heat competent to cover the solar emission for 2,000 years.' Nor can we question that whatever energy may correspond to the velocity, mass, and distance of a meteoric body at any epoch must have been transferred to the Sun if at some later epoch the mass of the meteor has come (after whatever processes) to

form part of the solar globe. Now, without committing myself to the opinion that the whole solar emission can be accounted for by combining these two causes, I must yet express the conviction that to forget the reality of these causes, their competence to account for some aliquot portion (let its amount be what it may) of the solar energies, would be a mistake. Nor can I see any valid reasons for asserting positively that the two causes combined may not account for a very large proportion of the Sun's activity. The irregular and perhaps intermittent supply of meteoric matter affords doubtless but an insufficient explanation of the Sun's copious and steady emission of heat; but the process of contraction would act the combined part of a 'governor' and an independent source of heat. Checked during the arrival of large meteoric supplies, and proceeding more rapidly when those supplies were temporarily diminished, it would account for that observed steadiness of emission which forms so important a characteristic of solar action.

I feel that I cannot bring this chapter more aptly to a conclusion than by quoting that noble passage in which Tyndall closes his discussion of the same subject:—'Presented rightly to the mind,' he says, 'the discoveries and generalisations of modern science constitute a poem more sublime than has ever yet been addressed to the imagination. The natural philosopher of to-day may dwell amid conceptions which beggar those of Milton. So great and grand are they, that in the contemplation of them a certain force of character

is requisite to preserve us from bewilderment. Look at the integrated energies of our world—the stored power of our coal-fields, our winds, and rivers; our fleets, armies, and guns. What are they? They are all generated by a portion of the Sun's energy which does not amount to the two-millionth of the whole. This is the entire fraction of the Sun's force intercepted by the Earth, and we convert but a small fraction of this fraction into mechanical energy. Multiplying all our powers by millions of millions, we do not reach the Sun's expenditure. And still, notwithstanding this enormous drain in the lapse of human history, we are unable to detect a diminution of his store. Measured by our largest terrestrial standards, such a reservoir of power is infinite; but it is our privilege to rise above these standards, and to regard the Sun himself as a speck in infinite extension—a mere drop in the universal sea. We analyse the space in which he is immersed and which is the vehicle of his power. We pass to other systems and other suns, each pouring forth energy like our own, but still without infringement of the law which reveals immutability in the midst of change, which recognises incessant transference or conversion, but neither final gain nor loss. The law generalises the aphorism of Solomon, that "there is nothing new under the Sun," by teaching us to detect everywhere, under its infinite variety of appearances, the same primeval force. To Nature nothing can be added; from Nature nothing can be taken away; the sum of her energies is con-

stant, and the utmost man can do in the pursuit of physical truth, or in the applications of physical knowledge, is to shift the constituents of the never-varying total. The law of conservation rigidly excludes both creation and annihilation. Waves may change to ripples and ripples to waves—magnitude may be substituted for number and number for magnitude—asteroids may aggregate to suns, suns may resolve themselves into floræ and faunæ, and floræ and faunæ melt in air—the flux of power is eternally the same. It rolls in music through the ages, and all terrestrial energy—the manifestations of life, as well as the display of phenomena—are but modulations of its rhythm.'

CHAPTER IX.

THE SUN AMONG HIS PEERS.

We have hitherto regarded the Sun with reference to his position in the solar system—as ruler, fire, light, and life of that wonderful scheme whose real magnificence and complexity have but recently begun to be recognised by astronomers. We have now to consider him as a member of the Sidereal System. What he is to the scheme of dependent worlds we have seen; it remains that we should endeavour to form some conception of his position among his peers. We have to contemplate him as a Sun among many suns, exerting an influence indeed over his fellow orbs, but, swayed in like sort by their attractions, still surrounded—as when we considered him with reference to the solar system—by orbs travelling with enormous velocity, but no longer at rest, or almost at rest, amidst a scheme of moving worlds. We are to see him taking part in a scheme of movement too wondrously complicated to be as yet interpreted by astronomers.

We must not pause here to consider the processes—interesting though their history may be—by which astronomers have been enabled to determine the distances of certain stars, and so to form a general esti-

mate of the scale on which the sidereal syst
structed. Let it suffice to mention that t
mental fact on which our estimate of the di
the fixed stars from us and from each other
based, is the circumstance that while our Ear
round the Sun on an orbit more than 18
miles in diameter, the stars remain all but u
in their apparent position, all the powers of ou
instruments only revealing in a very few inst
minutest conceivable displacement. Setting
clearly before us, the grandeur of the siderea
becomes more real and present to our mind:
the nearest fixed star, the vast orbit of our
reduced to little more than a point—to a
minute that 2,000 such circles could be place
side along the apparent diameter of the Sun
But the great majority of the stars lie at dist
vaster: so vast indeed, that the Earth's orbit i
to a mere point as viewed from beyond
abysms which separate us from those orbs.
likely that in general, the distance of star f
of any star in the heavens, for instance, f
nearest of its neighbours, falls short of the di
which our Sun is separated from the near
fellow orbs.

We are thus brought face to face with a
full of interest but enormously difficult—a
which belongs perhaps rather to the astronon
future than to that of our day. How are m
termine the figure and dimensions of the side

tem, to understand its structure and complexities, to trace out the motions taking place within its limits, when as yet they seem to have scarce any means of even attempting to solve these problems? Yet here, unless I mistake, is a work from which future astronomers will not shrink, a problem whose solution (for it *will* be solved) cannot but reveal results altogether surpassing in interest any which astronomers have yet obtained. It is true that if we consider the means we have for attacking this noble problem, they seem ineffective indeed; if we look at the results of past research we find little to encourage present confidence. Yet it is only necessary to consider the amazing interest of the problem to set doubt and irresolution on one side, and at least patiently to test the means we have at our disposal.

I have elsewhere * pointed out reasons for regarding the views formerly accepted respecting the sidereal system as unsatisfactory. I must avoid here, however, all reference to arguments already enforced, and indeed I propose but to sum up here the results I have exhibited elsewhere, and then to pass on to consider one special circumstance connected with the Sun's relation to his fellow suns—the *proper* motion by which he speeds through interstellar space.

Sir William Herschel, fired with the noble thought of gauging the celestial depths, took as the funda-

* In *Other Worlds than Ours*, and more especially the second edition, where additional, and, I think, conclusive arguments are brought forward. See also *The Universe*, where the evidence derived from my equal-surface chart of 324,000 stars is considered.

mental hypothesis on which his gaugings wer
the conception that the stars are spread with
general uniformity within a definite region o
If this one hypothesis be admitted, it becomes
by means of a telescope powerful enough to r
most distant and the smallest stars of the sy
gauge the extent of the system. All that is n
is to count the number of stars seen in the t
field of view when the instrument is directed
different parts of the heavens. Where many
seen, there the system must necessarily have its
extension; where there are few stars, there th
of the system must be nearest to us.

It is well known that by this process of star-
Sir William Herschel was led to adopt for aw
conclusion that the sidereal system has the f
cloven disc. The extension of this disc is tow
region of the heavens occupied by the Milky W
parts of the heavens where no milky light is s
responding to the flattened sides of the disc.
ing to the essential principle of this method
gauging, applied to the observed numerical r
it follows inevitably that the stars visible to th
eye lie far within the limits of the cloven dis
same conclusion follows, also, from Sir John H

* It is sometimes added that Sir William Herschel supp
tain general uniformity of size and brilliancy to exist amon
This, however, is a mistake. The only general hypothesi
Sir William Herschel was the one stated above; he added, a
to the admission of his own star-gaugings, the theory th
scope reached—at least in most directions—the limits of t
system.

gauges of the southern heavens; though his view of the sidereal system differed in *this* respect from his father's first view, that he considered the stars visible to the naked eye, and others down to about the tenth magnitude, to be less richly spread through space than those whose united lustre produces the milky light of the galaxy. But whether the richer parts of the sidereal system form a cloven disc in space, or a cloven ring, surrounding the lucid stars—in either case, accepting only Sir William Herschel's original fundamental hypothesis, we are bound to admit that the lucid stars lie far within the limits of the sidereal system.* In whatever direction we turn our eyes to look upon the lucid stars, we may be quite certain, if only this hypothesis be true, that the bounds of the star-system lie far beyond the constellations we are regarding.

Now it is at this point that my study of the stars has led to the recognition of evidence which opposes itself in the most striking manner to the views usually accepted. I find among the lucid stars the most convincing signs of aggregation along certain definite regions, and of segregation from others. I have applied to these signs the strictest principles of mathematical calculation, in order to determine whether they can by any possibility be due to chance distribution, and I find that it is wholly impossible so to interpret

* For convenience, astronomers speak of the stars visible to the naked eye as the *lucid stars*. The title has no reference, it will be understood, to the intrinsic brilliancy of the light of the visible stars.

them. But in this result, regarded by itself, tl
in truth nothing opposed to the theories fo
accepted. It is indeed an interesting circum
that such traces of aggregation and segregation
be recognisable, and perhaps it may seem to m
perplexing circumstance that these signs sho
long have escaped recognition.* But, apart fr
interest thus attaching as I think to the disc
there is nothing which may not be conceived to
very well with former views. For such peculi
of structure, if one may so speak, within the sph
the lucid stars might extend throughout the wh
the 'cloven-disc star-system,' and yet the averag
which Sir William Herschel based his earlier c
sions might not be disturbed. It is when anothe
most unexpected relation is mentioned that the
book theories are found to fail. The aggregat
stars distinctly recognised in some regions and
marked in others is most marked of all alon
Milky Way. Not only are lucid stars so richly s
on the Milky Way that for the whole heavens to
richly spread 6,000 new lucid stars would be w
but the gaps and lacunæ in the Milky Way s
bare and vacant that were the whole heavens no
4,600 stars now visible would have to be blotted

* This may be ascribed wholly to the strange nature of t
atlases hitherto constructed, in which the authors seem to have
how they might best (by distorting the celestial spaces and by
them over with monstrous figures of men and animals) conce
gether from view any laws of association which may really exis
the stars.

our view. Such, briefly stated, is the statistical evidence on this point. There can be no question that it is of the most convincing character. The probabilities against such a result if chance distribution were alone in question—that is, if no real relation existed between the lucid stars seen amid the milky light of the galaxy and the clustering groups of telescopic stars which produce that light--may be readily shown to be so overwhelming that no illustration can be devised to convey an adequate idea of their immensity. The chance that the Sun will rise to-morrow is ridiculously small (at least as Quetelet calculates it) by comparison. So that as long as the laws of probability are to be our guide in such matters (and in every scientific conclusion ever yet adopted we have had no other evidence) it must be regarded as certain that the lucid stars seen on the Milky Way are for the most part immersed among the crowds of minute stars forming the diffused light of the galaxy. These galactic stars then are much nearer than had been supposed, and they are *really* minute, not reduced merely to apparent minuteness by the vastness of their distance.

When we add to the considerations thus suggested that the nebulæ have been shown by unmistakable signs of association to form a system intimately connected with the system of stars, we begin to see that the sidereal system regarded as a whole is very different from that scheme of suns pictured in the old theories. Our Sun and his fellow suns are [illegible] with groups of minor suns, with clusters [illegible]

with masses of star-mist. We trace amid the system thus disclosed the signs of as yet unth laws. Here the large suns gather into wel clusters; here they form streams amid the depths. In one region we find them associa that strange spiral of minute stars forming the (see fig. 96); in another they are grouped

FIG. 96.

The Milky Way regarded as a Spiral.

crete nebulæ; and yet elsewhere they are i amid the whorls and convolutions of nebulous Lastly, in two regions we see suns and min star-clusters and discrete nebulæ, and masses lous matter combined into vast spherical agg —the Magellanic Clouds of the seaman—a aggregations themselves forming the centre markable group of lucid stars. This group, n

* Such is at least a figure which (as shown in my *Ot* accounts in a satisfactory manner for all the observed pec the Milky Way.

more than 2,500 orbs, covers one half of the southern heavens. It sweeps in a mighty spiral* around the greater Magellanic Cloud. It gathers its host of lucid orbs so densely along one part of its course that that region of the heavens alone suffices to light up the southern skies as with the light of a young moon. It presents, in fine, phenomena which leave little room for question that it forms a great and distinct system, within whose bounds are included all the characteristic features of the sidereal system itself, if indeed we are not to regard it as forming the noblest half of that portion of the universe of which we have hitherto become cognisant.

It is not without a purpose that I have thus directed the reader's attention to the vast southern star-system which constitutes the most striking and instructive feature of the heavens. If there is no feature of the northern heavens which to ordinary vision seems to correspond to this southern star-system, yet statistical research reveals the fact that the southern region has its true analogue in our northern heavens. The widely extended group of stars surrounding the projection of the Milky Way in Cepheus, and including within its limits the singularly rich portion of the Milky Way in Cygnus, has not only well-defined limits, but presents a well-marked superiority to the rest of the north-

* The features here referred to are strikingly exhibited in the Isographic Maps of the Northern and Southern Heavens accompanying the second edition of my *Other Worlds*. These maps show all the stars visible to the naked eye (in white on a black ground) truly distributed area for area.

ern heavens as regards richness of star-distribut
Though smaller in extent, it is not less rich on
average than the great southern rich region. It
responds also with that region in some other
rather peculiar respects. It covers a region wh
the Milky Way throws out projections and shows
vacuities. The Milky Way reaches it on one side
a single stream, on the other as a double stream;
further, the brightest portions of the Milky Way
the northern and southern heavens, lie near these
rich regions, and both also towards that edge of
rich region whence the double stream of milky li
extends.

Now, I would invite attention to the circumsta
that the Sun's proper motion, according to the b
estimates hitherto made, is carrying him from
borders of the southern rich region towards the bord
of the northern rich region. He is passing away fr
the neighbourhood of Canis Major, Columba, a
Lepus (not to define too precisely the as yet scarc
determined path along which he travels), and he
urging his way with inconceivable velocity towards
region between Hercules and Lyra. Of the true ha
tudes of those regions of space through which he
bearing, and has lately borne, his family of planets
know little. But as we look back along the extend
track he has pursued, and see the richness of th
regions he has left, and as we look onwards and tra
his course in imagination towards the borders of th
rich region whose glories gather into their chief

53 degrees to the plane of the Earth's orbit, and
15 degrees in advance of ♈.* Along this path the
pursues his course at a rate which has been estim
at about 150 millions of miles, or five-sixths of
diameter of the Earth's orbit in each year. He
since the Earth's orbit-plane is carried along at
rate, while the Earth circles around that orbit on
each year, it follows that the actual path pursue
the Earth in space† is such as is indicated in fig
It is in fact a skew spiral or helicoidal path.‡

The other members of the planetary system pu
paths of different figures. The coils of the heli
traversed by Mercury and Venus lie relatively
together; those of Mars are not so near as the coi
the Earth's path. The asteroidal helicoid pat
amazingly complicated they must be—are yet
drawn out (so to speak). But it is when we
beyond the asteroidal orbits that we get the mos
tended helicoids. Jupiter is carried some 1,700,000
miles onward with the advancing Sun, while he

* Its projection on the ecliptic, that is, lies in longitude 28
thereabouts.

† Or rather within the sidereal system, which itself doubtle
some motion—perhaps an inconceivably rapid motion.

‡ Some persons have expressed great anxiety lest, if the S
really travelling so swiftly through space, he should leave the
behind. There is not the least fear of this, any more than th
that the Earth should leave the Moon behind. Whatever forces
caused the Sun to follow his present career have acted upon the
and all the planets with equal effect. And as the Sun's course th
space becomes modified by the varying attractions to which h
be subjected, every planet in the solar system, every satellite, n
planetary comet, and asteroid, will experience the same influence
accompany the Sun just as faithfully as at present.

cuits once around his orbit of less than 1,000,000,000 miles in diameter; Saturn sweeps on through some 4,400,000,000 miles, while circuiting his orbit, less than 1,850,000,000 miles in diameter; and the paths traversed by Uranus and Neptune amid the depths of sidereal space are even more remarkably drawn out, regarding them in their helicoidal character.

As the Sun travels through space the planets sweep

FIG. 98.

The Earth's motion through space.

onward with him.* But has he, besides his planetary dependants, any companions on his voyage? Do any of his brother suns travel along with him? As yet we have no means of knowing, for a strange difficulty arises. If the Sun has companions, these must, of course, be relatively near to him. It does not follow that they will appear brighter than other stars, because

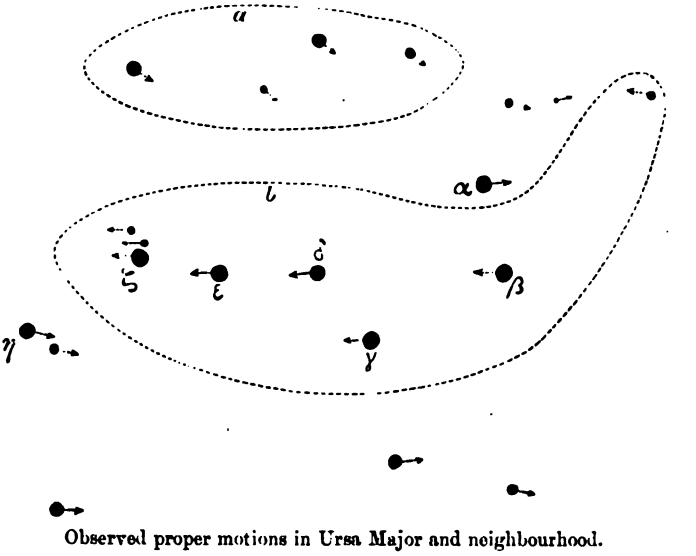
* It is worthy of notice that the Sun's northern hemisphere travels forwards. Is it not conceivable that in this peculiarity we may find some explanation of the greater heat which has been said to be emitted from the northern solar hemisphere?

if they are no larger than the Sun, we know that other orbs (as Sirius and Arcturus) must largely exceed them in real size, and so their relative proximity may not be rendered apparent. But this is not all. The stars which astronomers select as most likely to afford measurable indications of proximity are those whose apparent motions on the heavens are exceptionally large. Now the companions of our Sun on his voyage through the sidereal system doubtless travel on a nearly parallel course; and therefore, setting aside their orbital motions around each other, or around the common centre of gravity of the family, they must appear, as viewed by us, to be almost at rest. They would of course indicate in a more marked manner than any other stars the effect of the Earth's annual motion (or, technically, they would have a large annual parallactic displacement); but then astronomers would not be led to look for such effects, since we know as a matter of fact that the stars hitherto examined for signs of annual parallax are those which, either through exceptional brilliancy or through exceptionally large proper motion, seem likely to be near to us.

We find signs in the heavens leading us to regard the existence of such 'companions of the Sun' as at least not wholly improbable. Here, for example, is a picture (fig. 99) borrowed from my 'Other Worlds,' in which the chief stars of the constellation Ursa Major are depicted, and some few others belonging to Draco and Boötes. To each star is attached a small arrow indicating the direction of its motion, and the

amount of such motion in 36,000 years. We see here decided signs of star-drift. We can scarcely doubt that the five principal stars of Ursa Major included within the dotted line are travelling together through space; while the four stars above, belonging to Draco,

Fig. 99.



Observed proper motions in Ursa Major and neighbourhood.

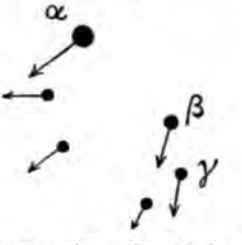
seem similarly to be companion suns. The remaining stars may also not improbably form a single family.

The group of stars shown in fig. 100 seem to form a system within which probably there are orbital motions of considerable magnitude.

It is worthy of notice that in the two instances here referred to there are evidences of association apart from the observed proper motions. For in the second

the stars forming the system seem segrega somewhat marked manner from neighbouri groups. The stars in Ursa Major, again, h noted by Fr. Secchi as having very similar s in other words, as resembling each other ver in structure and condition. In this circums

Fig. 100.



Observed proper motion of Stars in head of Aries.

have a peculiarity which may one day enab select with some confidence those stars which Sun's special companions, which voyage alo him through the sidereal system, and share w perchance in a reign over some special do space.

APPENDIX.

TABLE I.

Principal Elements of the Sun.

Equatorial Horizontal Parallax at mean distance from the Earth	8″·6
Diameter in miles	850,000
Diameter (Earth's as 1)	108
Volume (Earth's as 1)	1,260,000
Mass (Earth's as 1)	318,000
Density (Earth's as 1)	0·250
Density (Water's as 1)	1·42
Surface (Earth's as 1)	11.650
Gravity at Surface (Earth's as 1)	27·1
Fall of Bodies in feet in one second	436·3
,, ,, miles ,,	0·085
Greatest apparent Diameter viewed from the Earth	32′ 36″·41
Mean ,, ,,	32′ 3″·64
Least ,, ,,	31′ 31″·79
Linear value of 1″ at mean distance from earth	440 miles.

The Elements of the Sun's Rotation are given in Chapter IV. pp. 208–210.

TABLE II.

For determining the Effect of Changes in the Value of the S
torial Horizontal Parallax (at his mean distance) on th
mean distance.

Parallax (in seconds)	Mean Distance (in miles)	Difference (in mi
8·0	102,173,020	126,1
8·1	100,911,600	123,0
8·2	99,680,900	120,0
8·3	98,480,020	117,2
8·4	97,307,620	114,4
8·5	96,162,840	111,8
8·6	95,044,640	109,2
8·7	93,952,200	106,7
8·8	92,884,540	104,3
8·9	91,840,920	102,0
9·0	90,820,470	99,8
9·1	89,822,420	97,6
9·2	88,846,100	95,5
9·3	87,890,780	93,5
9·4	86,955,740	91,5
9·5	86,040,440	89,6
9·6	85,144,200	87,7
9·7	84,266,400	85,9
9·8	83,406,540	84,2
9·9	82,564,060	82,5
10·0	81,738,420	

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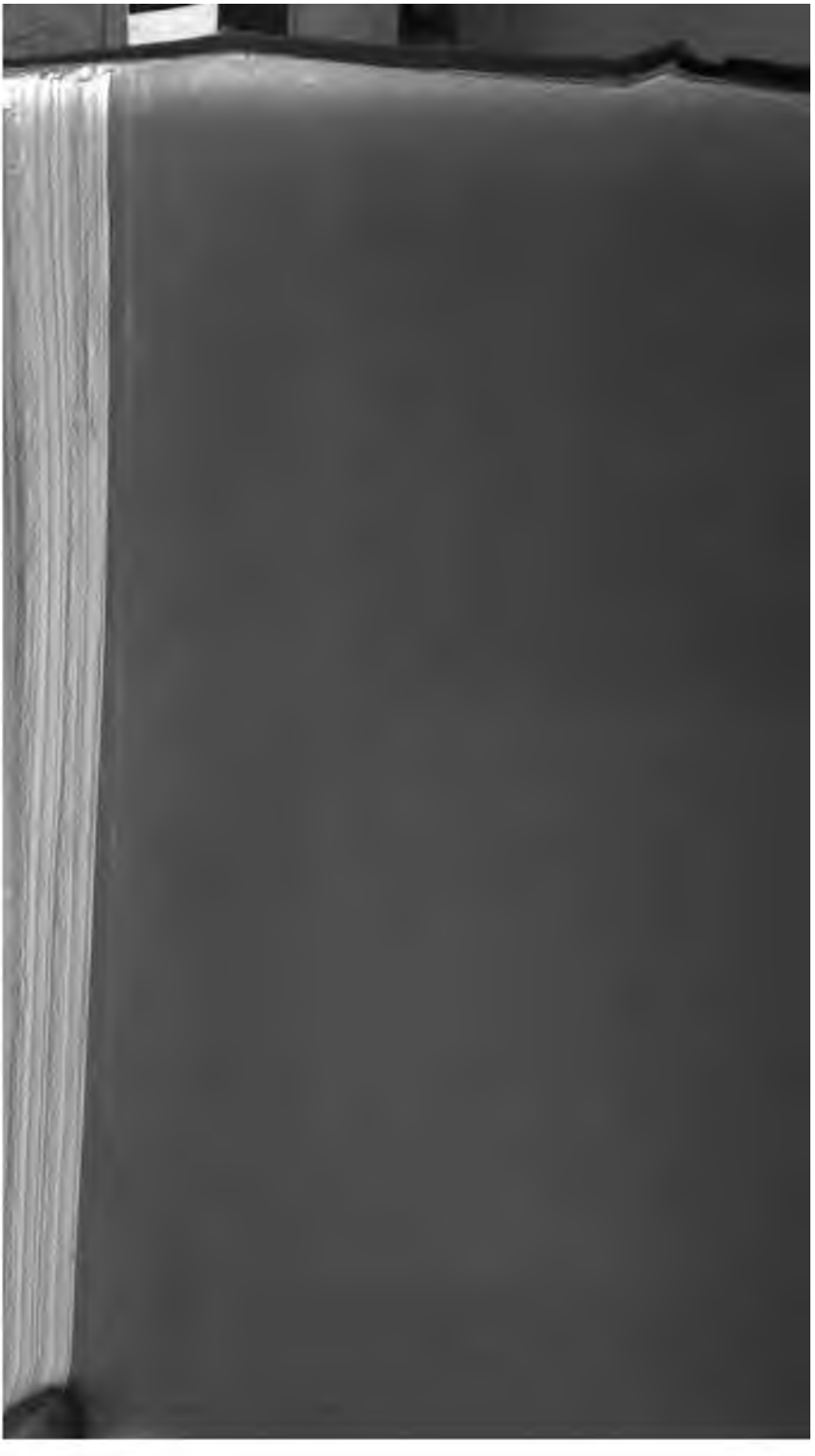
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